# Interlocking Directorates and Competition in Banking*

Guglielmo Barone
University of Bologna

Fabiano Schivardi
LUISS University and EIEF

Enrico Sette
Bank of Italy

July 5, 2024

**Abstract**

We study the effects on corporate loan rates of an unexpected change in the Italian legislation which forbade interlocking directorates between banks. Exploiting multiple firm-bank relationships to fully account for all unobserved heterogeneity, we find that prohibiting interlocks decreased the interest rates of previously interlocked banks by 14 basis points relative to other banks. The effect is stronger for high quality firms and for loans extended by interlocked banks with a large joint market share. Interest rates on loans from previously interlocked banks become more dispersed. Finally, firms borrowing more from previously interlocked banks expand investment, employment, and sales.

*Keywords*: Interlocking directorates, competition, banking
*JEL classification numbers*: G21, G34, D34

We thank Bob Chirinko, Luigi Guiso, Harald Hau, Luis A. Lopez, Gy-ongyi Loranth, Steven Ongena, Marco Pagano, Jose-Luis Peydro, Tuomas Takalo, Emanuele Tarantino, Jean Tirole and seminar participants at the Bank of Italy, ECB, EIEF, LUISS University, the University of Padua, the University of Vienna, MFA2023, CEPR Swiss Conference in Financial Intermediation, CEPR Adam Smith Workshop 2023 for useful comments and suggestions. Schivardi thanks financial support from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 835201). Email: g.barone@unibo.it, fschivardi@luiss.it, enrico.sette@bancaditalia.it. The views expressed in the paper are our own and do not necessarily reflect those of the institutions we are affiliated with.

> *"The practice of interlocking directorates is the root of many evils. It offends laws human and divine. Applied to rival corporations, it tends to the suppression of competition and to violation of the Sherman law." (Brandeis, 1914).*

> *"Section 8 [of the Clayton Antitrust Act] is an important, but underenforced, part of our antitrust laws. [...] Competitors sharing officers or directors [...] facilitate coordination – all to the detriment of the economy and the American public. The Antitrust Division is undertaking an extensive review of interlocking directorates across the entire economy and will enforce the law." (Assistant Attorney General Jonathan Kanter, Justice Department's Antitrust Division Press Release Number 22-1222 of October 19, 2022).*

Interlocking directorates (IDs henceforth) occur when two or more corporate boards of directors share one or more board members. Louis Brandeis, Associate Justice of the Supreme Court of the United States and President Wilson's chief economic adviser, actively campaigned against IDs, arguing that they reduce competition. In fact, they might help the boards of interlocked firms to coordinate pricing policies in overlapping markets. They could do so explicitly, sharing the information about the pricing policies of the two corporations, but also implicitly, as appointing a director already sitting on the other firm's board can in itself signal the intention to coordinate pricing. Following this idea, the Clayton Act forbids IDs between companies "that are [...] competitors such that the elimination of competition by agreement between them would constitute a violation of any of the antitrust laws." Japan and South Korea also forbid IDs if they harm competition. In Europe, where pro-competition policies have a more recent tradition than in the US, IDs are not specifically regulated but rather managed by the general competition law.

In practice, limits to IDs and outright bans are not strictly enforced and there is widespread evidence that they are very common in the corporate sector around the world. In a seminal paper, Dooley (1969) showed that 233 of the top 250 US corporations had IDs in the 1960s, a number slightly larger than that registered by a study of the National Resources Committee in the 1930s. More recently, Hauser (2018) analyzes the companies that comprise the S&P 1500 index from 1996 to 2014 and finds that roughly one-third of directors hold multiple appointments. Graham (2020) shows that, in 2016, a company in the S&P 1500 was connected, on average, with four other S&P 1500 companies through IDs. Nili (2020) analyzes data on all directors in the S&P 1500 in 2016 and finds that 27% of companies had at least one director on their board who also serve on another board *within* the same four-digit SIC code.[1] It is therefore not surprising that the Justice Department has recently announced "its intent to reinvigorate Section 8 enforcement" (the ban of IDs, see the quotation above).

One important problem when discussing the regulation of IDs is that there is very little empirical work that rigorously investigates their effects on competition. In fact, supplying causal evidence on this issue turns out to be very difficult. First, one needs exogenous variation in the structure of board connections, as sharing a board member might simply reflect, for example, the similar skill needs of firms operating in similar markets and therefore adopting similar policies, independently from collusion. Second, it is essential to have precise measures of market outcomes to assess how they vary with shocks to interconnections. Third, finding a suitable control sample for interlocked firms is tricky. For example, using firms in the same sector raises issues of treatment spillovers, while firms in other sectors might be on different trends. As a consequence of these empirical challenges, we know little about the causal effects of board interconnections on product market competition.

In this paper, we investigate the effects of IDs on competition in an ideal testing ground: the Italian corporate lending sector in the early 2010s. Our setting addresses all the challenges listed above. First, we exploit an exogenous change in the structure of bank interconnections triggered by an unexpected law of 2011 that forbade IDs among competing banks, breaking board connections. Second, we study the market for corporate loans, where we can precisely identify competing banks. Third, we leverage on the structure of this market, in which not only each bank lends to multiple firms, but also firms typically borrow from multiple banks. This allows to flexibly control for both fixed and time-varying unobserved heterogeneity between treatments – defined as the loans

[1] Similar conclusions are reached by Fich and Shivdasani (2006), who look at firms that appear in the 1992 Forbes 500 lists of largest corporations, and Heemskerk, Fennema, and Carroll (2016), who study the 300 largest European corporations.

of interlocked banks before the reform – and controls. Fourth, we use a database with detailed information on individual firm-bank lending contracts, so that we can observe firm-bank individual loan prices and their evolution before and after the reform, to test if prices of treated loans decreased after the reform relative to controls. We now illustrate these elements in more detail.

Our first ingredient is the policy change. The "Save Italy" decree of December 2011 (also known as the Monti Decree, named after the prime minister who issued it) obliged bank board members to resign from multiple appointments by the end of April 2012. As we explain below, the reform was totally unexpected and managed to overcome the opposition of the financial sector only because Italy was on the verge of default due to the Eurozone sovereign debt crisis. The reform was also very effective in drastically reducing bank connections to very few cases.

The second step entails defining treated loans. Following the antitrust authority, we use Italian provinces as the relevant geographical market for business lending (NUTS 3 units, broadly comparable to a US county). Within the province, we define a network of interlocked banks as a set of banks with IDs and with a sufficiently high market share to exert market power. Then, we label loans of the banks belonging to a network as treated.

We apply a standard difference-in-differences (DiD henceforth) framework, comparing the change in the interest rate on treated and control loans before and after the reform. Our setting allows for a very careful identification of the causal effects of IDs on loan rates. First, we exploit the exogenous and unexpected change in connections induced by the law, overcoming the problem of endogenous network formation and breakup that plagues most of the existing empirical literature, that is, assortative matching of similar banks which therefore apply similar pricing policies. Second, we exploit the specific features of the lending market to control for unobserved heterogeneity potentially correlated with the treatment. In fact, banks typically lend in multiple provinces, and given that the treatment is defined at the bank-province level, the loans from the same bank can be treated in one province and controls in another. Moreover, firms too typically borrow from more than one bank, so that some of their loans can be treated and other controls. This implies that we can run our regressions with a full set of firm-period and bank-period dummies, accounting for *all time-varying unobserved heterogeneity at the firm and at the* *bank level*. That is, identification comes from analyzing the evolution of the within firm-period and within bank-period difference in rates on treated and control relationships. This allows to fully account for shocks that hit both the firm and the bank, as well as for any other time-varying confounding effect, such as other measures contained in the Monti law that might affect differentially firms and banks, and be correlated with the treatment. We also control for unobserved (time-invariant) characteristics of the bank-firm relationship, thus accounting for matching effects. To the best of our knowledge, no other paper can implement such a rigorous empirical design to identify the causal effects of corporate interconnections on competition.

We find that the severance of IDs has a pro-competitive effect: in our preferred specification, the interest rate on treated relationships drops by 14 basis points relative to the controls (1.5% of the average interest rate on treated loans before the treatment). In terms of size, the effect is in the ballpark of recent work on the effects of common ownership on airline tickets as well as of the effects on interest rates of recent bold expansionary monetary policy measures undertaken by the European Central Bank. An event study shows no evidence of a pre-trend. The effect is significant starting from the fourth quarter after the reform, and after 3 years the drop in interest rates reaches 29.2 basis points (3.1% of the average rate).

We conduct a large series of robustness checks. We modify the market share thresholds used to define the treatment. We also experiment with the number of pre and post-periods, as well as with a closed sample to account for the possible influence of attrition. We repeat the analysis at the firm (rather than at the firm-bank) level, addressing potential issues of reallocation of credit demand across lenders. In all cases, we fully confirm the findings.

The theory of collusion predicts that prices are less dispersed in a collusive equilibrium than under competition.[2] Consistently, we show that interest rates on previously interlocked relationships are less dispersed before the reform and that dispersion increases after it. We then run a series of comparative static exercises based on firm and network characteristics that we expect to be related to the strength of the effect. In terms of firm

[2]The market for corporate lending can be thought of as a differentiated product market, in which price dispersion would be present even if banks fully compete.

characteristics, we find that more creditworthy firms record a larger drop in rates. We interpret this as indicating that, once the market becomes more competitive due to the reform, such firms can exploit more intensively their better outside option and therefore renegotiate loans more aggressively. In terms of network characteristics, we find that the drop in the interest rates is larger for loans whose network had a higher market share or was sustained by a large number of IDs before the reform.

While we focus on anti-competitive effects, IDs might also allow for information sharing, possibly reducing the extent of asymmetric information on borrowers. If this were the case, the breakup of IDs might restrict credit supply due to rationing (Stiglitz and Weiss, 1981; Crawford, Pavanini, and Schivardi, 2018). To assess this possibility, we look at the quantity of credit granted by banks. We find no evidence of reductions in granted credit on treated relationships after the reform, against the hypothesis that IDs might benefit customers by reducing the extent of asymmetric information. Finally, we consider the real effects of the reform. We find that firms with a larger share of credit from treated relationships experience higher investment rates, employment growth and sales growth in the post-reform period. Overall, we conclude that the reform was instrumental in improving the performance of the corporate sector.

Our paper contributes to the debate on the anti-competitive effects of firms' interconnections, focusing on IDs. The literature that documents the diffusion of IDs shows that they are pervasive even within sector and argues that this is an important reason of concern (Dooley, 1969; Hauser, 2018; Faleye, Hoitash, and Hoitash, 2011; Fich and Shivdasani, 2006; Heemskerk, Fennema, and Carroll, 2016). However, until recently, direct evidence on IDs' effects on competition was basically non-existent. Three recent contemporaneous papers address this issue. Geng, Hau, Michaely, and Nguyen (2021) use the staggered adoption at the state level of a change in the U.S. corporate law to construct an instrument for IDs. They provide evidence that they contribute to reducing competition, particularly for R&D intensive firms. Colombo (2022) studies the US Airline market and shows that, when two directors share an appointment in a third, non-competing company, the two competing airlines curtail supply and increase prices. For US listed firms, Gopalan, Li, and Zaldokas (2022) use a similar approach to address causality and show that new IDs between product market peers increase profitability and prices. These results are in line with ours. Like Geng et al. (2021), we exploit a change in the corporate law as a source of exogenous variation in IDs. Like Colombo (2022), we have a very precise measure of prices and of overlapping markets. In addition, we exploit a special feature of the corporate loan market–the fact that both banks and firms entertain multiple lending relationships–to control flexibly for all unobserved heterogeneity.

A series of papers document a positive effect of inter-industry board connections on different measures of performance, such as M&A transactions (Cai and Sevilir, 2012), risk-adjusted stock returns (Larcker, So, and Wang, 2013), cost of debt (Chuluun, Prevost, and Puthenpurackal, 2014), innovation activities (Chang and Wu, 2021). Faia, Mayer, and Pezone (2022) study the effects of the "Save Italy" decree (the same that we use) on the stock market returns of Italian listed corporations, including non-financial ones (although the ban on IDs affected only banks and insurance companies), finding that reducing network centrality of a firm depresses returns. While the general result that IDs have a positive effect on performance is consistent with ours, the reason, and therefore the implications, is very different. These papers focus on inter-industry IDs and argue that the positive effects derive from the fact that they provide useful information to firms. We focus on intra-industry IDs and supply causal evidence of their anti-competitive effects. In this sense, our results speak to the competition policy issue of IDs regulation, stressing the importance of distinguishing between inter- and intra-industry interlocks.[3]

A recent body of work focuses on common ownership, whose importance has substantially increased over time, as a potential anti-competitive mechanism (see, among others, He and Huang, 2017; Azar, Schmalz, and Tecu, 2018; Koch, Panayides, and Thomas, 2021; Lewellen and Lowry, 2021). Compared to this literature, we focus on a different mechanism of interconnection. Our empirical design also fully addresses the identification challenges that plague the literature.

The rest of the paper is organized as follows. In Section 1 we shed light on the mechanisms underlying the impact of IDs on the market outcome and illustrate the

[3] Azar and Vives (2021a) also stress the different effects of firms' interconnections, defined in terms of common ownership, within and across industries. Specifically, they build a model which shows that intra-industry common ownership always increases prices, while inter-industry common ownership can actually decrease them due to the general equilibrium effects of an intersectoral pecuniary externality. They test these predictions with data from the airline industry, finding supporting evidence (Azar and Vives, 2021b).

"Save Italy" decree. Section 2 introduces the definition of the network and explains the empirical setting. Section 3 describes the data, while Section 4 reports the results and the robustness exercises. Section 5 tests additional implications and performs the heterogeneity analysis while Section 6 considers the credit quantity and firm performance. Section 7 concludes.

# 1 Shared board members, collusion and the Monti Decree

In this section we first discuss the economic mechanisms through which shared board members (SBMs henceforth) might facilitate collusion and then describe the content of the Monti Decree.

## 1.1 How can SBMs facilitate collusion?

Article 36 of the Monti decree, which we illustrate in detail in the next subsection, forbids SBMs among competing financial institutions, explicitly motivating this ban with the objective of increasing competition in the financial sector.[4] The implicit assumption is that IDs can contribute to sustaining collusive behavior between banks. We now discuss the potential mechanisms through which this can happen.

First, note that IDs are not randomly assigned to banks but rather chosen by the banks themselves. Banks can strategically choose to appoint a director already sitting on the board of competing banks exactly because they intend to collude and the choice of connecting two boards with a SBM could directly be a signal of that intention. However, collusive equilibria must be sustainable. In this respect, IDs can be particularly useful, since SBMs constitute a direct channel of information sharing between competing banks. The exchange of information between competitors is a factor that facilitates sustaining the collusive agreement, especially in markets such as the credit market, where the prices charged by competitors are not readily observable (Harrington Jr and Skrzypacz, 2007). Indeed, when prices are unobservable, the detection of deviations from the collusive strategy may be problematic, making punishment not credible (Stigler, 1964; Green

[4] The title of the article is "Protection of competition and personal cross appointments in the credit and financial markets".

and Porter, 1984).[5] Below, we provide evidence that SBMs are more likely to hold an executive position in one of the banks compared to non-shared directors. This reinforces their credibility in terms of supporting the collusive equilibrium, as executive directors have both more information about the bank's strategies and more power to determine them. If the connection is broken, this channel of coordination and information exchange is removed. This may make the collusive equilibrium unsustainable and prices will fall.

The next question is the level at which coordination takes place. A first possibility is that a SBM can directly identify shared customers and coordinate the pricing of individual loans of the interlocked banks. While theoretically possible, the relevance of this mechanism is limited by the fact that only loans above a certain threshold go through the Board of Directors.[6] Moreover, large loans are typically granted to large firms, that borrow from multiple banks and also have access to other sources of external finance other than bank credit, therefore limiting the capacity of banks to exert market power even when interlocked.

A more likely hypothesis is that coordination occurs at the local market level. Below we provide evidence that there is a province component of bank-level pricing. The board can contribute to the determination of prices at this level. SBMs can be instrumental in the coordination of less aggressive policies in markets in which the interlocked banks have a substantial overlap. Any deviation from the collusive strategy would be immediately detected by the SBM.

Since local loan officers play a role in determining loan conditions, particularly on small loans, an important question is how collusion at the board level is passed down to branch loan officers, who might not even be aware of IDs. The answer is that the bank can affect individual loan officers' pricing decisions. In fact, even when granting autonomy

[5]Other theoretical insights point to slightly different mechanisms that explain the value of information sharing to sustain collusive equilibria. Kandori and Matsushima (1998) argue that the exchange of information between rival firms improves the estimation of market demand, which in turn facilitates the maintenance of a coordinated behavior by allowing firms to infer whether a decrease in individual demand is due to the defection from the collusive agreement by competitors or to a negative shock at the market level. Farrell (1987) focuses on cheap talk as a tool to achieve coordination when the game has multiple equilibria.

[6]A survey run by the Bank of Italy finds that the maximum amount a local branch manager can grant autonomously increases with the size of the bank. The average is around 550.000 euros for managers of larger banks and around 200.000 and below for smaller ones (Albareto, Benvenuti, Mocetti, Pagnini, Rossi et al., 2011).

to loan officers, the headquarters can set market-specific general rules for price setting. For example, the price might be a combination of soft information collected by the officer and hard information evaluated centrally, using market-specific scoring models. The headquarters can also affect loan officers' pricing decisions by differentiating across local markets the cost or the quantity of funds provided through the internal capital market.[7] They can also set different incentive schemes in terms of targets for corporate lending, setting less aggressive targets for officers operating in markets in which the interlocked banks jointly operate.

## 1.2 The Monti decree

Our analysis exploits the so-called "Save Italy" decree (Law Decree 201/2011 of December 6, 2011). The decree was passed as part of the effort of the newly appointed Monti government to avert the risk of Government default and "Italexit" from the Euro Area. It aimed at improving the long-run Government financial sustainability to restore the confidence of financial markets in the Italian government debt. It entailed three broad lines of intervention. First, cuts to public expenditure, most notably through the reform of the pension system. Second, increases in taxes, particularly the value added tax and the real estate tax. Finally, measures to foster the long-run growth of the country. Among these measures (that include eliminating some barriers to competition, such as restrictions to opening hours in retail trade and to entry and conduct of pharmacies) feature those contained in Article 36, that forbid any individual to hold simultaneous appointments in the governing bodies (boards and other top management positions) of two competing banking groups.[8] In what follows, for brevity sake we refer to groups as "banks", given that our analysis is a the group level, and use "individual banks" to indicate single banks within groups (for stand-alone banks, the two terms coincide). Two banks

[7] The idea that the headquarters can indirectly affect the pricing decisions of middle managers by affecting the marginal costs of production was introduced in the contest of the common ownership literature by Antón, Ederer, Giné, and Schmalz (2023), who also provide empirical evidence supporting this mechanism.

[8] In principle, the norm applies also to top managers who are not on the board of directors. However, in our data all individuals with two or more appointments are members of at least one board. The typical case is an individual with a top managerial position in bank $j$, without belonging to bank $j$'s board, who also sits in the board of bank $k$. Given that these individuals hold an executive position in bank $j$, despite not being directly part of the board, they can clearly affect the strategic choices of both banks and we will simply refer to them as SBMs.

are defined as competitor if they operate in the same local market. After the reform, individuals holding more than one appointment in competing banks as a consequence of a new appointment must opt for only one office and resign from the other(s) within 90 days. In the transition period, the choice had to be made within 120 days (by the end of April 2012). Throughout the paper, we assume that Q4-2011 is the last quarter before the policy (passed in December 2011), instead of Q2-2012 (the deadline within which the banks had to comply with the new regulation) to avoid anticipation effects.

The chain of events that led to the approval of the decree makes it an ideal quasi-natural experiment, as the policy was completely unexpected and it led to the exogenous breakup of bank connections. During the summer of 2011 the sovereign debt crisis erupted throughout Europe. Italy was badly affected, with the spread between the Italian and the German 10 year government bond yield increasing from 150 basis points before the summer to above 500 by the end of the year. Both financial markets and European institutions exerted a strong pressure on the Italian government to undertake reforms to increase the growth potential. Against this background, the Berlusconi government resigned on November 12, 2011, and four days later the Monti government took office. The government was formed mostly by non-politicians and had the explicit mandate to undertake structural reforms and bring the budget under control to ease the tension on the sovereign debt. The "Save Italy" decree, as the name itself suggests, was the first strong signal of the Italian commitment to remain in the euro. It was drafted under very strong time pressure and approved less than a month after the government took office. Its content, specifically the one we exploit, was totally unexpected for both banks and firms. Arguably, only the dramatic situation the country was going through allowed the government to approve measures like those banning IDs that would have been very hard, if not impossible, to approve in normal times, due to banks' lobbying activity.

Despite the fact that Article 36 was not the only measure contained in the law to improve growth, our identification strategy isolates the effect of this specific channel. First, Article 36 is the only one that affects banks directly. Second, and more importantly, our empirical framework, illustrated in the next section, allows us to control for any observable and unobservable determinants of credit conditions that could be correlated with other measures contained in the decree, as well as with other concurrent shocks that materialized over the period.

# 2 Empirical design and identification

In this section, we first describe our definition of treated loans and then illustrate the identification strategy.

## 2.1 Defining treated loans

Our definition of treatment is at the bank-market level. As typical in the banking literature, we define markets in terms of geographical units.[9] This choice is based on a large body of empirical evidence, which underlines the local nature of the corporate lending market, especially for small firms like most of those in our sample. For example, Petersen and Rajan (2002) and Degryse and Ongena (2005) point to the existence of a banker's rule of thumb, according to which banks prefer to lend to customers located within 4 and 1.4 miles of a branch, respectively. Using a sample comparable to ours, Crawford, Pavanini, and Schivardi (2018) show that firms are on average 2.9 km (1.8 miles) away from the nearest branch of their main bank. Scholars rationalize the proximity between borrowers and lenders as a tool to facilitate information acquisition and reduce travel costs for borrowers. In Appendix B we provide direct evidence of the local component of lending markets, showing that banks tend to specialize geographically, that there is an important bank-local market component in interest rates, and that the degree of geographical specialization is much stronger than that of sectoral specialization.

The geographical component of banking markets is embraced by the regulatory bodies, which typically use provinces as relevant markets. For example, when entry was regulated, the Bank of Italy relied on provinces to decide whether to authorize the opening of new branches. The Italian Antitrust authority typically considers the province as the relevant geographical market in banking, in particular when assessing M&As.[10] Based on this evidence and practice, we also use the Italian provinces, administrative units roughly comparable to US counties, as our definition of geographical market. In Italy, there are

[9]As stated above, Article 36 of the Monti decree explicitly defines competing banks as those operating in the same geographical market.

[10]See https://www.agcm.it/competenze/tutela-della-concorrenza/operazioni-di-concentrazione/lista-concentrazioni/.

110 provinces, and the average radius, after approximating its surface with a circle, is 30 km.[11]

Next, we discuss the definition of networks of connected banks. Connections are between boards of individual banks. Around one-third of individual banks have at least one SBM (1.4, on average). Each connected individual bank is connected, on average, with three other individual banks (two if we only consider individual banks belonging to different banking groups). However, as stated above (see Section 1), the law bans board connections at the *group* level, so we define connections at the group rather than at the individual bank level. Specifically, the link between group $j$ and $k$ exists when there is a common board member between at least one individual bank belonging to group $j$ and one individual bank belonging to group $k$.[12] When two groups ("banks" in the terminology define above) are connected, all individual banks belonging to the groups are connected. In the last quarter of 2011, 125 banks were connected, out of a total of 604 banks.

In a nutshell, we build our definition of treatment based on three conditions: two or more banks are connected via IDs; they operate in the same province with non-negligible individual market shares; and they account for a "sufficiently large" provincial market share. We now describe in detail how we implement these conditions.

(i) <u>Board connection</u>. We start by requiring that banks be connected through IDs. Consider the case in which bank $j$ is connected to one or more banks (say $k$ and $l$). We say that banks $j$, $k$, and $l$ are jointly connected and we call bank $j$ "pivotal", as it is directly connected to the other banks. Note that banks $k$ and $l$ are not necessarily directly connected: it is sufficient that both are directly connected to bank $j$.

(ii) <u>Lending in the same province with non-negligible individual market shares</u>. Bank

[11] Provinces are typically used as the relevant markets in the empirical banking literature using Italian data (Sapienza, 2002; Guiso, Pistaferri, and Schivardi, 2012).

[12] It might be that two large banking groups are linked because two small individual banks, each belonging to a group, share a board member. In such a case, coordination at the group level is not obvious, and we might be misclassifying some loans as treated. Note that this would play against finding an effect of IDs. Reassuringly, in our data board connections involve the main bank in the group in 79% of cases. Below we show that our results are confirmed when assuming that two banking groups are connected if and only if the ID involves the holding banks in the groups.

$j$ and at least one of its connected counterparts must serve the same province $p$, each with a non-negligible market share. This condition is imposed to avoid assigning the treatment status to connected banks in $p$ in which there is a bank with a high market share and all the others with very small market shares. For example, suppose that the market share of bank $j$ in province $p$ is 20%, and that of banks $k$ and $l$ is 0.1%. In such a case, breaking the connection is unlikely to affect the pricing behavior because even before the policy banks $k$ and $l$ were adding very little market share to that of the dominant bank. In our data, the supply side of the market is rather fragmented as the distribution of market shares at the province-bank level, computed in the year before the Monti law (from Q1-2011 to Q4-2011), has a large mass of density in the left part of the distribution. We require a minimum market share of 1% (approximately the $87^{\text{th}}$ percentile of the market share distribution at the province-bank level). As we show below, our core results are confirmed both if we do not impose any minimum threshold and if we raise it to 2% ($91^{\text{th}}$ percentile).

(iii) <u>"Sufficiently large" aggregate provincial market share</u>. The aggregate market share of banks satisfying conditions (i) and (ii) must be "sufficiently large" to give these banks the capacity to affect market prices. Choosing this threshold is tricky, as there is no obvious way to determine the market share above which the network is able to exert market power. Some guidance can be gained from the Antitrust Authority. The general rule of the Authority is that, in case of an M&A, involved banks *may* be required to dismiss branches in local markets in which the market share of the merging banks exceeds 15% (Lotti and Manaresi, 2015). In practice, in the three years before the reform, the decisions of the Antitrust Authority suggest that the actual threshold above which the provision is enforced is around 35%.[13] Note however that, in an M&A, the Authority trades off increases in market power with efficiency gains, so that it should be willing to accept an increase in market

[13] The lowest new entity's market share relative to cases in which the authority had concerns about competition is in the [35%-40%] interval ("Intesa San Paolo/Banca Monte Parma" case, 2011) while the highest new entity's market share relative to cases in which the Authority had no concerns about competition is in the [30%-35%] interval ("Banca Cassa di Risparmio di Firenze/Banca Monte dei Paschi di Siena" case, 2010).

power if mitigated by the cost savings induced by the M&A. This suggests that 35% might be too conservative to detect situations in which banks exert market power. We address this problem using a robust approach. We set the cutoff at 20% in our baseline specification, at which 29% of the lending relationships are classified as treated, and show that our results hold, and change in the expected way, if we modify this threshold to either 10% (41% of relationships classified as treated) or 30% (10% of relationships classified as treated).

We define a set of banks satisfying conditions (i)-(iii) in province $p$ as a *network in* $p$. Note that, in each province, a bank can belong to more than one network, as illustrated in Appendix Figure A3. If bank $j$ belongs to at least one network in $p$, we set the (time-varying) dummy $D_{jpt} = 1$ for all loans this bank issues in province $p$ at $t$.[14] Figure 1 shows that, after the reform, the relevance of IDs in the local credit markets collapses. We report the time pattern of the provincial market shares of banks for which $D_{jpt} = 1$, normalized to one in Q4-2011. We use the market share to take into account both the existence and the local relevance of IDs. For comparison, we also report the total number of bank-provinces. Against a rather stable path for both variables in the year before the policy, the share of loans for which $D_{jpt} = 1$ drops discontinuously when the reform becomes effective and quickly converges to around a 95% reduction in the post-period, while the total number of bank-provinces declines only marginally and smoothly.[15]

Finally, we determine the quarters in which the conditions stated above must hold to classify a relationship as treated. In fact, $D_{jpt}$ can vary across quarters due to the formation or dissolution of links and because the market shares might cross the thresholds defined above, particularly for banks/networks close to the respective thresholds. For our analysis, both situations are problematic. First, IDs formation and breakup before the law may be endogenous. Second, changes in $D_{jpt}$ due to small changes in the market shares around the thresholds do not represent real changes in networks. To account for both issues, we define the time-invariant treatment status $TR_{jp} = 1$ if $D_{jpQ4-2011} =$

[14] We compute the network characteristics for the single bank (total number of banks in the network, cumulative market share, etc., see below) as the average across that bank's networks.

[15] Results are similar if instead of summing up market shares corresponding to bank-provinces with $D_{jpt} = 1$ we simply count them. Note that the indicator for bank-provinces with $D_{jpt} = 1$ does not go to zero either because some connections between very small banks can be in place or because, irrespective of banks' size, multiple appointments are possible as long as they last less than 90 days.

$D_{jpQ3-2011} = D_{jpQ2-2011} = D_{jpQ1-2011} = 1$, that is, a loan extended by bank $j$ is treated if the conditions above hold in *all* four quarters before the Monti law. Analogously, we define $TR_{jp} = 0$ if $D_{jpQ4-2011} = D_{jpQ3-2011} = D_{jpQ2-2011} = D_{jpQ1-2011} = 0$. Loans treated only in some of the four quarters ending in Q4-2011 are dropped from the sample. We show that the results are robust if we instead define $TR_{jp}$ on the basis of three or five periods before the reform, or if we use a time-varying treatment before the reform, fixing the value for the post-period to that of the last period before the reform, that is $TR_{jpt} = D_{jpt}$ in the pre-period and $TR_{jpt} = D_{jpQ4-2011}$ in the post-period (in this case, no loan is dropped from the sample).

## 2.2 Identification

We analyze the effects of the reform using a DiD framework which, exploiting the features of our setting, allows us to carefully address several identification challenges that can affect our estimates. Our dependent variable is the interest rate $r_{ijpt}$ that bank $j$ charges to firm $i$ located in province $p$ in period $t$. Our preferred measure is the gross interest rate, which includes fixed costs such as fees and commissions, as market power can be exploited by increasing such components (Sufi, 2009). We show below that results are similar using the net interest rate. The basic regression equation is the following:

$$r_{ijpt} = \alpha_0 + \alpha_1 POST_t + \alpha_2 TR_{ijp} + \alpha_3 TR_{ijp} \times POST_t + \boldsymbol{\alpha}_4' \mathbf{X}_{ijpt} + D_{ijpt} + \epsilon_{ijpt} \qquad (1)$$

where $POST_t$ is a dummy for the post-period (from Q1-2012 onward), $TR_{ijp}$ is a dummy for treated relationships as defined above, $\mathbf{X}_{ijt}$ is a vector of firm, bank and firm-bank time-varying characteristics and $D_{ijpt}$ denotes various combinations of fixed effects used in different specifications. Interpreting $\alpha_3$ as the causal effect of the breakup of IDs is challenging, as there may be many potential sources of unobserved heterogeneity correlated with both the treatment and the outcome. In what follows, we explain why we believe that our empirical design can robustly account for basically any potential correlated effect.

The first fundamental identification challenge that plagues the empirical literature on network effects is endogenous network formation and breakup. In our contest, banks might share a board member as a consequence of the fact that they have similar customers or adopt similar pricing policies, questioning the causal interpretation of the treatment. Similarly, changes in market strategies and in pricing policies might lead to changes in the board composition and therefore in the endogenous breakup of networks. Due to the unexpected policy change, in our framework the breakup of IDs was exogenous – i.e., not chosen by the banks but mandated by law – and unanticipated. This ensures that the treatment is not an endogenous banks' response to some characteristics of their portfolio of customers or to a shock that directly affects the interest rates.

The exogenous network breakup is a necessary but not sufficient condition to identify the causal effects of IDs on interest rates. First, one might be concerned with correlated *fixed* bank and firm attributes or aggregate *time effects*. For example, firms with interlocked relationships might be different from the others, if anything because the probability of having interlocked relationships increases with the number of relationships a firm has, which in turn is correlated with size. Moreover, interest rates have an obvious time component. To address these concerns, we exploit our DiD framework and include in all regressions firm, bank and quarter fixed effects.

The estimation of equation (1) with individual and quarter fixed effects is the standard setting for DiD exercises. However, it leaves open the possibility that fixed firm and bank attributes have time-varying effects, which would invalidate the causal interpretation of $\alpha_3$. This concern is particularly important in our setting. Specifically, there is evidence that the Eurozone debt crisis, which prompted the sudden adoption of the ban on IDs (and of other measures discussed in Section 1.2) had differential effects on firms and banks according to their size or their financial strength: small firms usually suffered more than large ones during the crisis, and banks' lending policy were heavily influenced by their capital ratios (Chodorow-Reich, 2014; Schivardi, Sette, and Tabellini, 2021) and funding structure (Cingano, Manaresi, and Sette, 2016). These characteristics might be correlated with the treatment and therefore induce a spurious correlation between the treatment and the interest rate. To continue with the example above, suppose that, for some reason, small firms are less likely to have treated relationships.[16] If the deterioration of their performance leads to an increase in the interest rate relative to large firms, we would

[16]For example, because small firms are more likely to borrow from small, local banks, which might be less likely to have IDs.

observe a negative value of $\alpha_3$: large firms, more likely to have treated relationships, record a drop in interest rates relative to small ones, and we would erroneously interpret the drop as an effect of the reform. The typical fix is to interact firm characteristics with the post dummy. However, there might always be unobserved characteristics that we do not control for and that bias our estimates. The special features of the business loans market and the richness of our data allow us to fully tackle this important threat to identification. In fact, firms generally entertain multiple banking relationships (see the data description below). This implies that, following the seminal idea of Khwaja and Mian (2008), we can include a full set of firm-quarter dummies, a standard identification tool in the literature using credit registry data.[17] In this specification, we exploit only within firm-quarter variability in interest rates. In particular, identification comes from the within-quarter comparison of rates on loans that a firm obtains on treated and control relationships: stated differently, the control sample is made of the relationships that the same firm has with banks that do not belong to any network in the local market (province). The same concerns in terms of time-varying unobserved heterogeneity apply to banks. In this case too, we can capture any time-varying effect on rates by a full set of bank-quarter dummies. Consider bank $j$, whose loans are classified as treated in province $p$, where the bank belongs to a network, but not in province $q$, where it does not. The treatment dummy turns on only for loans that bank $j$ extends in province $p$, while those in province $q$ end up in the control group. Conditional on the bank-quarter effects, $\alpha_3$ is estimated as the difference in the change in interest rates on treated with respect to control relationships of the same bank. Note that this framework also fully controls for other potential confounding factors, such as other measures included in the "Salva Italia" or in other policy interventions that might have differentially affected firms or banks. To pose a threat to identification, such confounding factors should apply differentially to treated and control relationships, *within bank-quarter* and *within firm-quarter*. In

[17] Paravisini, Rappoport, and Schnabl (2020) question the validity of this identification strategy to account for credit demand shocks, as firms might differentially direct their credit demand to banks with different characteristics, possibly correlated with the demand shock. This is not a problem in our setting, as the severance of connections induced by the law is an exogenous shock defined at the level of bank-province, and therefore uncorrelated to shifts in a firm's credit demand. In fact, we see no reason why the exogenous breakup of the connection between two banks should be correlated with any shock specific to customers of each bank in the provinces in which they jointly operate. As argued by Paravisini, Rappoport, and Schnabl (2020), this is a sufficient condition for the identification strategy to be valid.

particular, the confounding factor should affect only the loans a firm obtained by banks we classify as treated at the provincial level.

One could also be concerned with province-quarter effects that are correlated with the intensity of treatment at the provincial level. For example, a province might be particularly affected by the downturn generated by the sovereign debt crisis and also have a particularly high presence of treated relationships. Again, given that in the same province we have both treatment and control relationships, we can fully account for this with a set of province-quarter dummies. Note that our preferred specification with firm-quarter dummies directly controls for province-quarter shocks, as firms are only located in one province.[18]

A final concern is that there might be features specific to the firm-bank match, above and beyond separate firm and bank (also interacted with time) effects. For example, treated relationships might entail a higher degree of information on the bank's side exactly because treated, which might in turn affect the bank's lending policy to the treated firms compared to other firms. These differences are taken care of by our DiD design. However, the same unobserved heterogeneity at the match level could imply different attrition rates in the post-period, implying that the estimating sample changes over time in a non random fashion, possibly biasing the coefficients. To account for this possibility, in our most saturated specification we also add firm-bank fixed effects. In a series of robustness exercises, we will also check if the estimates change when focusing on different samples, such as a closed sample of relationships that exist throughout the estimating period.

To sum up, our identification is based on the exogenous breakup of interconnections and on the possibility to fully account for fixed and time-varying confounding factors both at the firm and at the bank level. To the best of our knowledge, no paper in the literature on the anti-competitive effects of firms interconnections can implement such a robust empirical strategy.

The average number of firm-bank relationships is only slightly higher in Italy and the average share of credit from the main bank is slightly lower than the average. Finally,

[18] One degree of heterogeneity we cannot control for is at the bank-province-quarter level, as bank-province-quarter dummies would absorb the Treated and Treated*Post dummies. To threaten our identification, there would need to be shocks that hit banks' lending policies differentially across provinces, and such shocks should be correlated with the treatment. We could not think of any plausible story supporting this hypothesis.

some key board characteristics of Italian banks, such as size, the share of independent directors, and the share of non-executive or outside directors, align with those of other countries.

Panel A of Figure 3 shows the distribution of the number of networks in provinces. Approximately one-quarter of provinces have no networks, over 40% have only one network, and less than 5% have four or more networks. Panel B shows that nearly two-thirds of networks consist of two banks, one-quarter consist of three banks, and the remainder consist of four banks.

In 82 provinces there is at least one network. Conditional on having at least one network, the average number of networks is 1.7, and the banks in a network have a cumulative market share of almost 33%.

# 3 Data description

Before detailing the data used in the estimation, we show that the Italian credit market is similar to those of other developed economies. Table 1 reports comparative statistics for some key banking indicators for France, Germany, Italy, Spain, the UK, and the US. In terms of the number of banks, Italy falls in the middle of the distribution. Regarding financial development, the private credit-to-GDP ratio is similar in Italy, France, and Germany, substantially higher in Spain and the UK and lower in the US. Italy is also in the middle of the distribution in terms of banks' concentration, efficiency, and profitability indicators. The average number of firm-bank relationships is only slightly higher in Italy and the average share of credit from the main bank is slightly lower than the average. Finally, some key board characteristics of Italian banks, such as size, the share of independent directors, and the share of non-executive or outside directors, align with those of the other countries.

We draw upon four distinct data sources for our investigation. The first is the register of bank board members, managed by the Bank of Italy (the Or.So. database), which allows us to identify individuals serving on boards of directors of different individual banks at the same time. Next, data on individual banks and on banks at the group level, including market shares at the provincial-quarter level used to compute if a bank/network is above the thresholds to define the treatment, come respectively from the unconsolidated and consolidated balance sheets of the Supervisory Reports that banks submit to the Bank of Italy. The information on the group structure is taken from the Bank of Italy too. The third data source is the Italian Credit Register, providing quarterly information on loans granted and drawn, as well as the interest rates charged by banks operating in Italy. Interest rate information is available for a subsample of banks for relationships in which the total lending to the firm is above 75,000 euros. Banks exempt from the requirement to submit interest rate information to the central bank are mainly small and operating in narrow local markets. As of December 2011, information on interest rates is available for 81% of the total volume of credit in the credit register. Banking groups for which at least one bank provides information on interest rates account for 91.1% of total assets held by Italian banks. We select revolving credit lines ("credit lines" in what follows) because they have a series of desirable features for our analysis. First, the bank can change the interest rate at any time. This allows the shock to market structure to show up quickly in prices. Second, credit lines are standardized–unsecured with no fixed maturity–enabling meaningful price comparisons. Third, many firms hold multiple credit lines, permitting the inclusion of firm-time effects.[19] Finally, information about firms comes from Cerved, a data provider that supplies balance sheets and income statements for all incorporated companies. We match the Credit Register with firms in the Cerved database and keep observations for which we observe balance sheet data for the borrower.

Descriptive statistics on directors' characteristics are reported in Table 2. At the end of 2011, when the decree was passed, 1.95% of directors had more than one appointment (138 out of 7,066). On average, SBMs were more likely to be male (indeed, *all* SBMs were male), college graduates, older (64 vs. 60 years), hold an executive role, and had a longer tenure (1,096 vs. 794 days). In terms of job title, multiple-appointment directors were disproportionately President, Vice-President or CEO/General Director (see Figure 2), suggesting that SBMs held significant influence in shaping critical firm decisions, such as

[19]Multiple credit relationships, even in the presence of heterogeneous prices, have been explained by the literature as a mechanism to ensure the firm's credit supply against bank-specific liquidity shocks (Detragiache, Garella, and Guiso, 2000). Moreover, despite standardized, there can still be some differentiation among credit lines, such as through complementary products offered by the bank or proximity to bank branches. Note that our empirical framework fully accounts for these potential differences, given that we control for firm-bank fixed effects.

pricing strategies.[20] Appendix Figure A1 shows the distribution of the number of SBMs across pairs of connected individual banks.[21] In most cases, only one SBMs connects two banks, but in some case the connection is based on two or more SBMs.

As explained above, the connection between individual banks triggers the connection between banks (i.e., banking groups). Appendix Table A1 reports descriptive statistics of bank characteristics separately for connected and non-connected banks. Connected banks are on average larger in terms of assets and use interbank funding more extensively. The differences in terms of ROA, liquidity over assets, and equity over assets are small.

Next, we illustrate the characteristics of networks, specifically groups of banks within a province that meet the conditions for their loans to be classified as treated. Panel A of Figure 3 shows the distribution of the number of networks in provinces. Approximately one-quarter of provinces have no networks, over 40% have only one network, and less than 5% have four or more networks. Panel B shows that nearly two-thirds of networks consist of two banks, one-quarter consist of three banks, and the remainder consist of four banks. Table 3 reports descriptive statistics of network characteristics, focusing on those on which we will perform comparative static exercises. The average network market share is approximately 30%. The average number of individual interlocked banks in each banking group that are part of a network is 1.3.[22] Within networks, banks' market shares are fairly concentrated (the Herfindahl index is on average about 6,000) and the difference in the market share of the largest and second-largest bank in the network is about 15%. This is relevant to gauge the ability of banks belonging to the same network to collude. Banks in the same network jointly operate on average in about 40 provinces. This is also important because the greater the number of shared markets (multi-market contacts), the more severely a bank that deviates from collusive behavior can be punished, as more markets are impacted (Bernheim and Whinston, 1990).

Table 4 describes local credit markets. The average number of banks in each province is 110. The large number of active banks derives from the fact that we define a bank

[20] Multiple-appointment directors are classified in the highest role they hold across the different boards. For example, bank $j$'s CEO who serves also as director in bank $k$ is classified as CEO.

[21] This statistic is computed at the level of individual banks rather than groups because boards pertain to individual banks and not to groups.

[22] As explained above, our unit of analysis is a banking group. Within a group, there can be more than one individual bank that shares board members with other individual banks of other network members.

active in a province if at least one firm borrows from it, without imposing the existence of a branch or a minimum market share. Only 14 banks have a market share of at least 1%, while the top 3 banks account for more than half of the local market. On average, there are 1,425 borrowers in each province. In 82 provinces there is at least one network. Conditional on having at least one network, the average number of networks is 1.7, and the average market share of networks is almost 33%.

Finally, we describe the data directly used in the regressions. The sample period ranges from Q1-2011 until Q4-2014 (16 quarters). The pre-period is made of 4 quarters and ranges from the beginning of the sample to Q4-2011, the last quarter before the policy.[23] The post-period lasts 12 quarters and spans from Q1-2012 to the end of the sample. We let the sample run until Q4-2014 to better detect longer-term effects of the reform, but we also run robustness checks on a shorter window. Given that we are interested in the DiD estimate of $\alpha_3$, in our basic specification we focus on the sample of relationships that existed in the last quarter of 2011. The sample includes 192,732 unique firms and 3,561,068 firm-bank-quarter relationships. There are 85,898 firms with at least one treated relationship, accounting for 1,046,785 bank-firm-quarter relationships, which means that 29% of the observations are treated. Appendix Figure A2 shows the share of treated relationships by province. While there are clusters in certain areas, these are spread across the whole country. Treated relationships are frequent both in the North and in the South and in provinces with very different economic and demographic structure.[24]

Appendix Table A2 reports descriptive statistics for the interest rate for treated and control relationships in the pre and post-period.[25] Controls relationships display a lower interest rate in the pre-period (9.14% vs. 9.31%). Rates on both treated and control relationships increase in the post-period, but by less on treated ones (1.12% vs. 1.35%). As a consequence, rates of treated and control relationships converge in the post-period.

Appendix Table A3 reports descriptive statistics for lending relationships, firms, and

[23]We select a relatively short time span because, before the reform, the treatment changes with the (endogenous) changes in IDs. For example, a loan that is treated in 2011 might have not been so in 2010 if the ID that generates the treatment started in 2011.

[24]The Italian economy is marked by a significant regional divide, with Southern regions historically experiencing lower levels of economic development, institutional quality, and social capital.

[25]We trim the observations when the interest rate is below 1% and above 30%, as these typically represent reporting mistakes. However, we check the robustness of the results to several alternative ways to winsorize or trim the data.

banks in 2011 (i.e., before the reform), distinguishing between treated and controls. The unit of observation is a lending relationship. This implies that the same firm (bank) can be both in the treatment and control group, as long as it has both treated and control relationships. The size of the loan is somewhat higher in treated than in control relationships. In terms of relevance for the firm, treated relationships represent on average half of the total credit obtained by the firm, conditional on having at least one treated relationship.

Firm characteristics indicate that our sample is very comprehensive as it includes a large fraction of small firms, in line with the structure of the Italian economy. Firm characteristics are rather similar across treated and control relationships. This is not surprising, as many firms have both treated and control relationships. To delve deeper into the potential selection of firms into treated and control relationships, we split firms into three exclusive groups: those with only treated relationships, those with only control relationships, and those with both treated and control relationships. Appendix Table A4, Columns 1-3, shows that firm characteristics are balanced across the three groups. The only exception is size, which is larger for firms with mixed relationships. This is expected, as the probability of having both treated and control relationships increases with the number of relationships, which is positively correlated with size. We have repeated the analysis conditioning on having 2 or 3 relationships respectively (Columns 4-9), in which case size is also balanced across the three groups (with a marginal exception for 3 relationships all treated). This shows the absence of selection into treatment. However, it is important to stress that, as explained above, our most saturated specification fully accounts for time-varying both observed and unobserved heterogeneity at the firm level, fully addressing this potential issue.

Bank characteristics are very similar across treated and control relationships: banks have a similar leverage ratio, reliance on wholesale funding, liquidity ratio, and profitability, with a difference emerging for size. This is in line with the descriptive statistics in Appendix Table A1 on connected and unconnected banks, despite the fact that there are two important differences in the way the statistics are computed. First, Table A3 is computed at the relationship level, so that each bank is weighted by its number of relationships, while Table A1 is at the bank level. Second, in Table A3 the same bank can be treated in some provinces and control in others, while in Table A1 a bank is either connected or not. Again, our most saturated specification fully accounts for time-varying both observed and unobserved heterogeneity at the bank level, fully addressing any potential issue arising from differences in bank characteristics.

# 4 Results

In this section we present the main results and then perform a series of robustness checks.

## 4.1 Main results

We estimate Equation (1) to determine the effect of the exogenous breakup of IDs on interest rates. We cluster standard errors at the bank-province level, which is the dimension at which the treatment varies.[26] Table 5 reports the results. Column (1) runs a parsimonious specification, in which we only include sector-quarter, province-quarter, firm and bank fixed effects. The interest rate on treated relationships in the pre-period is 29 basis points higher than on controls, and highly significant. It drops by 20 basis points after the law becomes effective, and the decrease is statistically significant at the 5% level. Moreover, we fail to reject that, in the post-period, the sum of the two coefficients is equal to zero (p-value 0.213), that is, that after the reform the interest rate of treated relationships fully converges to that on control relationships.

These patterns are confirmed in the other columns, where we increase the granularity of the controls. In Column (2) we add a standard set of firm controls: size (log of assets), leverage (debt over equity), ROA (EBIT over assets), liquidity (liquid assets over assets), and, as a summary measure of creditworthiness, a dummy equal to one for firms with an Altman Z-score in the three higher risk categories, out of a total of nine;[27] and bank controls: size (log assets), equity to assets ratio, ROA, interbank deposits (including repos) to assets, liquidity over assets (firm and bank controls are unreported for brevity). Firm and bank controls are lagged one period. The estimates of the treatment in the pre and post-periods increase in absolute value, to 0.37 and -0.32 respectively, and are

[26] Alternative clusters, such as bank-province-quarter level, deliver smaller standard errors.

[27] The Altman Z-score is computed by the data provider Cerved and is commonly used by banks to price loans (Rodano, Serrano-Velarde, and Tarantino, 2018).

significant at the 1% level. Again, we fail to reject the hypothesis of full rates convergence after the reform. The coefficients on firm characteristics are all significant and with the expected sign. The same holds for banks characteristics, with only the liquidity ratio and interbank funding not statistically significant.

Next, we fully exploit the granularity of our data to account for both observed and unobserved heterogeneity. In Column (3) we add firm-quarter fixed effects, which fully absorb both firm controls and firm, sector-quarter and province-quarter fixed effects. In this specification, the coefficient is estimated only by comparing the pre-post difference in the rate that a firm pays on treated relationships with the rate the same firm pays on control relationships within a given period. The coefficients are similar to those in Column (2), suggesting that unobserved heterogeneity at the firm-quarter level does not bias the estimates.

In Column (4) we add bank-quarter fixed effects, which absorb all the observable bank characteristics and also control for unobserved, time-varying bank heterogeneity. Again, this means that we only compare rates that the same bank charges across different provinces in which its loans are classified as treated or controls. This ensures that time-varying shocks at the bank level are fully accounted for. With bank-quarter dummies, the effects decrease somewhat: the average interest rate on treated relationships in the pre-period is 22 basis points higher, and it drops by 14 basis points in the post-period, both significant at the 1% level. Again, we fail to reject full convergence. Finally, in Column (5) we add firm-bank fixed effects to control for firm-bank idiosyncratic matching factors, so that only the Treated*Post coefficient can be estimated. The coefficient remains very stable (-0.14) and statistically significant at the 1% level. This regression is fully saturated: the estimate only exploits within firm-bank, firm-quarter and bank-quarter variability in interest rates, accounting for all possible heterogeneity that could be correlated with the treatment and singling out the effect of the reform.

The results of Table 5 are clear-cut and indicate that the prohibition of IDs reduced the rates on treated relationships. The estimated effect ranges from 32 (Column 2) to 14 basis points (Columns 4 and 5). It decreases slightly when introducing firm-period and especially bank-period fixed effects. This drop might signal the importance of accounting for unobserved heterogeneity. However, it might also be due to spillovers effects from treated to control relationships that should be taken into account when evaluating the effects of reform. For example, the drop in the coefficient when adding bank-period effects indicates that banks with IDs charge higher rates in the pre-reform period, and reduce them in the post-period, also in provinces in which they are not classified as treated below, we provide suggestive evidence against this hypothesis). This might be due to a bank level shock that we want to control for. But it might also signal a spillover effect from treated to control provinces. We therefore interpret the estimates of Column (5) as a lower bound of the effects of the reform, and that in Column (2) as the upper bound.

To analyze the time evolution of the effect, we estimate a version of Equation (1) in which the treated dummy $TR_{ijp}$ is interacted with a separate dummy for each quarter, using the most demanding specification of Column (5) in Table 5, and plot them in Figure 4, together with the 5% confidence intervals. The base quarter is the last quarter of 2011 and is identified by the blue bar. First, there is no evidence of a pre-trend in the interest rate on treated relationships: all the coefficients for the pre-period are small and statistically insignificant.[28] There is also no effect in the first quarter after the reform was passed, when the law was effective but banks had four months to comply with it. After that, the coefficients become negative and progressively larger in absolute value. They become statistically significant in the fourth quarter since the approval of the law, and keep decreasing throughout.

The estimates reported in Table 5 and in Figure 4 all indicate that the interest rate on treated relationships dropped after the inception of the Monti Decree. The size of the effect in the most saturated specification is around 14 basis points at the average (see Table 5) and 29.2 at the peak (see Figure 4). Given that the average rate for treated relationships before the reform was 9.3%, this amounts to a reduction in the rate of between 1.5% and 3.1% (3.6% and 7.5% of the standard deviation, respectively). In terms of comparison with the results from the common ownership literature, this reduction is not far from Azar, Schmalz, and Tecu (2018)'s estimate of the impact of common ownership on airline ticket prices in the US (3%–7%). In the banking industry, Cai, Eidam, Saunders, and Steffen (2018) study the role of the distance in specialization among lenders in the US

[28]The absence of a pre-trend is confirmed by a regression using the pre-period only, in which the interaction between the treatment dummy and the time trend is not statistically different from zero.

syndicated loan market, hypothesizing that closer syndicates might collude by exploiting the informational lock-in (Sharpe, 1990; Rajan, 1992). They find that a one-standard deviation drop in distance (i.e. lenders have similar specialization) reduces loan pricing by 5 basis points (2% of the average). In terms of size, our estimates are also in the ballpark of the estimated effects of recent bold expansionary monetary policy measures undertaken by the European Central Bank.[29]

## 4.2 Robustness

We now perform a series of robustness checks, focusing on the most saturated specification of Table 5. We start by assessing the relevance of the choices we make to define the treatment and then perform a series of additional checks, including the definition of the interest rate, the length of the post-period, the use of a closed sample and the definition of the network.

**Definition of the treatment.** Our definition of treatment rests on the two thresholds used to define networks at the provincial level: the 1% market share of the individual bank and the 20% aggregate market share of the network. We now check how our results change when selecting different thresholds. First, we modify the individual market share. Table 6, Panel A, Column (1) reports the results when setting the individual market share to 0%, that is, not imposing any minimum share for network members. We find that the drop in the interest rate for treated relationships is slightly smaller (-0.127 vs. -0.139 in Column (5) of Table 5), which suggests that when the market share is very low the effects might be slightly reduced. In Column (2) we increase the threshold to 2%, finding a slightly larger drop in the interest rate than in the baseline specification (-0.156).

Next, we modify the network's provincial market share threshold, set at 20% in the baseline. Column (3) reports the result when decreasing it to 10%, finding again an effect very similar to the baseline (-0.150). In Column (4) we increase it to 30%, in which case the drop is larger (-0.241). Finally, in Column (5) we experiment by setting both thresholds to zero, that is, for any positive value of the individual and aggregate market

[29]Benetton and Fantino (2021) estimate that banks that participated in the LTRO reduced rates on loans by 20 basis points more than other banks, while Bottero, Minoiu, Peydró, Polo, Presbitero, and Sette (2021) estimate that one standard deviation higher exposure of banks to negative policy rates leads to 40 basis points lower rates on overdraft loans.

shares. In this case, we still get a negative coefficient (-0.130) but we marginally lose statistical significance. Overall, these results suggest that the effect is already present even with no limit on market shares, and it increases somewhat when imposing more stringent requirements for both the individual and, especially, for the aggregate network market share.[30]

**Number of pre-periods.** The second element in the definition of treatment is the number of periods in which we require the indicator at the firm-bank-province-quarter $D_{ijpt}$ to be 1 for the relationship to be classified as treated ($TR_{ijp} = 1$). In the baseline we require 4 quarters, to ensure that the network was in place for a sufficiently long time to display its effects on lending relationships. In Panel B of Table 6 we experiment by modifying the length of this period. We always impose $D_{ijpQ4-2011} = 1$, that is, that the conditions to be classified as treated are satisfied in the last quarter before the reform, and modify the number of required preceding periods. In Column (1) we require that the network is active for the last 3 quarters before the reform, that is, $TR_{ijp} = 1$ if $D_{ijpQ4-2011} = D_{ijpQ3-2011} = D_{ijpQ2-2011} = 1$; in Column (2) we increase the requirement to the last 5 quarters (from Q4-2010 to Q4-2011). In both cases, loans treated only in some of the quarters ending in Q4-2011 are dropped from the sample. The results show that perturbing the number of periods in which we require the indicator $D_{ijpt}$ to be switched on does not alter the estimate of the effect of the reform on rates.

Next, we experiment with conceptually different definitions of the treatment. In Column (3) we adopt a time-varying definition, letting the treatment indicator free to switch on/off in the pre-period while freezing it at its Q4-2011 value in the post-period (that is, $TR_{ijpt} = D_{ijpt}$ up to Q4-2011 and $TR_{ijpt} = D_{ijpQ4-2011}$ from Q1-2012 on). With this definition, we allow for loans changing treatment status in the pre-period, thus avoiding the elimination of firm-bank relationships that do change status. The disadvantage is the risk of endogenous changes in the treatment status before the reform. These concerns do not seem to be very relevant in practice as the drop is basically identical to that of the baseline specification (-0.133 vs -0.139). Column (4) uses the same time-varying definition and considers a longer time span before the policy change (from 4 to 8

[30] As a sanity check, we have also verified that the effect is insignificant when we consider networks with small market shares.

quarters). In this case, the drop increases in absolute value, to -0.163. Finally, Column (5) combines an 8-quarter pre-period with a treatment that is time-varying in the first 4 quarters and then is set to the same baseline value in the last 4 periods. The estimate (-0.171) is rather similar to that of Column (4). Overall, the estimates are very robust to the number of periods required to define the treatment. They tend to get slightly larger in absolute value when the number of periods increases, possibly because networks that have been in place for a longer period are able to sustain a stronger coordination.

**Other robustness checks.** We now control the robustness of our results along a series of additional dimensions. First, we experiment with the definition of the interest rate. We chose the gross rate as our preferred measure because fees and commissions represent a relevant part of the total cost of loans and an important source of banks' income. Still, the net rate is also important, as it captures the marginal cost of credit, which, once a relationship is established, is the relevant cost measure for investment decisions. To check if our results are affected by this choice, we repeat the exercise using the net interest rate, which excludes fees and commissions. Column (1) of Table 7 shows that the effect is slightly smaller (-0.090), in line with the hypothesis that market power affects fees and commissions too, but significant at the 1% level, indicating that also the marginal cost of credit is affected.

In our preferred specification all firm and bank time-varying attributes are absorbed by dummies. However, we can still include time-varying characteristics of the relationship. In Column (2) we include the share of total credit to the firm by the bank and the share of overdraft loans out of total loans supplied by the bank to the firm. While the two additional regressors (not shown for brevity) are highly significant and negative, the drop in the average interest rate on treated relationships is -0.135, very similar to the baseline specification.

Next, we shorten the number of quarters in the post-period to 4 instead of 12, as one might argue that considering a longer period might confound effects not necessarily due to the reform. In addition, in this way, we have the same number of quarters in the pre- as in the post-reform periods. The estimate decreases slightly to -0.120 (Column 3), consistent with the evidence of Figure 4 on the evolution of the treatment effect, but remains highly statistically significant.

Another issue relates to sample selection. Our baseline specification uses the open sample, that is, with all relationships, independently from the fact that at some point some dissolve. The number of treated relationships, therefore, in the post-period shrinks over time due to attrition. This might induce selection bias in our estimates. As discussed above, this concern is greatly mitigated by the fact that we use firm-bank effects, which account for unobserved heterogeneity that is time-invariant at the relationship level. Still, one might argue that long lasting relationships have specific features that might make them respond differently to the treatment. To address this concern, we construct a closed sample, that is, we drop all relationships that at some point disappear from the sample. To avoid losing too many observations, we restrict the sample to the same 4 quarters in the post-period used in Column (3).[31] This substantially reduces the number of observations, to around half a million. Despite this, Column (4) shows that the estimate remains significant and similar to that in Column (3), suggesting that, conditional on our rich set of controls, selection is not an issue.

In Column (5) we drop the first two quarters of 2012, during which the banks had to comply with the regulation but could still have IDs. Consistently with the evidence of Figure 4, which shows little effect in those quarters, the estimate increases (in absolute value) to 15.8 basis points.

Our regressions so far are unweighted, and as such inform us of the average change in the interest rate, independently of the size of the credit lines. While this is the primary object of interest, the cost of credit for the firm does depend on the size of the credit line. To account for this, Column (6) repeats the basic regression using the contemporaneous share of used credit as weights. The estimate is very similar to the unweighted one (11.3 basis points), indicating that composition effects due to the size of the credit lines do not affect the results.

Our last robustness exercise considers the definition of the connection between banks. As explained in Section 2.1, the ID ban applies to banking groups, so we chose groups as our unit of analysis. A possible drawback of this choice is that it might happen that two groups are connected because two small banks belonging to it are connected. This is more

[31] Results not shown also hold if we run this test on a closed sample for 8 quarters after the reform.

questionable than the case in which the connection is between the two main banks in the groups. This concern is mitigated by the fact that, in our data, most of the connections (79%) are driven by IDs between the largest banks in the groups. In any case, to check for this possibility, in Column (7) we redefine the treatment imposing that two banking groups are connected if and only if the ID involves only the bank holding companies of the groups. We find an estimated coefficient that is basically the same as in the baseline.

# 5 Extensions

We now corroborate our main results with a series of extensions that also help to nail down the mechanism through which the ban of IDs affects interest rates.

## 5.1 Price dispersion

Different banks may have different information about the same borrower or assess the same information differently, in line with the heterogeneity in the rates to the same firm that we observe in the data. An implication of collusive behavior is that rates set by network members should be less dispersed than those offered by other banks, and that the dispersion should increase after the reform if the ban on IDs reduces the scope for collusive behavior. To test this hypothesis, we compute the standard deviation of the interest rates at the province-quarter level separately for treated and control relationships:

$$\sigma_{pct} = \sqrt{\frac{1}{n_{pct}} \sum_{ij \in pc} (r_{ijpt} - \overline{r_{pct}})^2},$$

where $p$ is the province where the firm is located, $c$ is an index for treated and control relationships, $pc$ is the set of relationships of type $c$ in province $p$, $n_{pct}$ is the number of relationships of type $c$ in province $p$ at time $t$, and $\overline{r_{pct}}$ is the average interest rate on such relationships. Note that, for each period, $\sigma_{pct}$ assumes two values in provinces in which there are both treated and control relationships, and one in the others. We then run the following regression:

$$\sigma_{pct} = \gamma_0 + \gamma_1 POST_t + \gamma_2 TR_{pc} + \gamma_3 TR_{pc} \times POST_t + Dum_{pct} + e_{pct}. \tag{2}$$

where $TR_{pc}$ is a dummy equal to 1 if $c =$ treated and $Dum_{pct}$ are different sets of dummies.

The results are reported in Table 8. In Column (1) we only include quarter and province fixed effects. Consistently with the theoretical predictions, in the pre-reform period the standard deviation is 25 basis point lower for treated relationships and increases by 7 basis point after it, implying that it partially converges to the same level of the controls (we reject the null of full convergence). In Column (2) we add province-quarter fixed effects to account for time-varying province shocks and province-treated fixed effects to control for fixed attributes at the province-treatment level. In this specification, in which the Treated dummy is absorbed and cannot be estimated, the point estimate of Treated*Post becomes larger (0.100 vs 0.067).

One potential issue with these estimates is that the characteristics of the pool of treated and control firms might differ, possibly leading to differences in the dispersion of riskiness of the two groups. This possibility would not explain why the dispersion drops for treated relationships in the post-period. However, to fully account for it, we also compute a measure of dispersion after accounting for firm level determinants of the interest rates. Specifically, we regress the interest rate on the large set of firm controls that we include in Column (2) of the baseline specification: ROA, liquid assets to total assets, leverage, log of firm assets, a dummy equal 1 if the firm has a Z-score in the 3 worst categories (out of 9). We then use the residuals of this regression to compute the conditional price dispersion. The results, reported in the next two columns, are similar to those based on the unconditional price dispersion, the only difference being that dispersion in treated relationships is slightly higher before the reform and the drop is slightly smaller after it.

We therefore conclude that, in line with predictions of a breakup in collusive behavior, interest rates on treated relationships are less disperse before the reform and become more disperse after it.

## 5.2 Heterogeneity

We now explore the heterogeneity of the effect of the ban on IDs in terms of characteristics of the firm and of the network. We do so using the following estimating equation:

$$r_{ijpt} = \beta_0 + \beta_1 TR_{ijp} \times POST_t + \beta_2 TR_{ijp} \times POST_t \times HET_{ij} + D_{it} + D_{jt} + D_{ij} + \eta_{ijpt} \quad (3)$$

where $HET_{ij}$ is the measure of firm or network heterogeneity and $D_{it}, D_{jt}, D_{ij}$ are firm-quarter, bank-quarter and firm-bank fixed effects respectively, which absorb all lower level interactions.[32] All interaction variables (mediators) are measured before the policy was passed. In particular, firm characteristics are taken from the 2010 balance sheets and network characteristics are the average of the 4 quarters of 2011 (consistently with the definition of the treatment).

**Firm characteristics.** We consider the five firm characteristics that typically affect the interest rate and that we have included as controls in Table 5, Column (2): size (log of assets), leverage (debt over equity), ROA (EBIT over assets), liquidity (liquid assets over assets), and dummy for risky firms. These indicators can loosely be interpreted as different measures of firm "quality", in terms of size and financial strength. Ex-ante, the interaction effect is not obvious. On the one hand, a "better" firm might get a more competitive rate before the reform, and therefore benefit less from it. On the other hand, once the degree of competition increases due to the breakup of IDs, firms that are more creditworthy might find it easier to renegotiate their terms and obtain lower rates. We construct a dummy LOW for firms with a value of the specific characteristic equal or smaller than the median (in this case the distribution includes both treated and control relationships). The coefficient measures the difference in the drop for these firms with respect to those with a value above the median. The results, reported in Table 9, Panel A, are clear cut: the drop is larger for "better" firms. Column (1) shows that the decrease in the interest rates is 10 basis points smaller for small firms. Column (2) shows that low leverage firms record a substantially larger drop (16 basis point, highly significant) than high leverage firms, for which the drop is only 7 basis points. The same pattern emerges for low ROA firms, for which the drop in the interest rate is 13 basis points smaller than high ROA firms (Column 3); for low liquidity firms, 12 basis points smaller (Column 4); and for risky firms (Z-score=7,8 or 9), 13 basis points smaller (Column 5).

This evidence unambiguously points to the conclusion that more creditworthy firms benefit the most from the reform. One possible explanation is that these are the firms where the credit generates the highest surplus, as they have better investment opportu-

[32] Specifically, $\text{LOW}_i$ and $\text{LOW}_i*\text{POST}_t$ are absorbed by firm-quarter dummies, and $\text{LOW}_i*\text{TR}_{ji}$ by firm-bank dummies.

nities. In a less competitive environment, banks are able to appropriate a larger part of this surplus. When competition increases, the bargaining power shifts towards the firms, and those that generate a higher surplus benefit the most.

**Network characteristics.** If IDs facilitate collusion, the drop in interest rates should be stronger for networks with characteristics more conducive to support a collusive outcome. We leverage this conjecture and extend our basic regression framework to include an interaction between measures of network heterogeneity and the Treated*Post dummy. As for firm characteristics, we construct the dummy Low equal to one for values of the specific characteristic smaller or equal to the median, computed on the population of treated relationships, and zero otherwise.[33] Table 3 reports descriptive statistics of such characteristics. Panel B of Table 9 reports the results. We begin with a measure of network market power, that is, the network total market share (Column 1). The interaction Treated*Post*Low has a positive coefficient of 0.177, significant at 1%. We interpret this as an indication that lower market share networks exerted lower market power and therefore increased interest rates less than high market share networks in the pre-reform period. After the reform, therefore, the convergence to the "normal" interest rate is achieved with a smaller drop. This result is consistent with that of Table 6, Column (4), where we increased the threshold for the market share in the definition of network, finding a stronger effect.

In Column (2) we test the prediction that tighter connections are more capable of sustaining collusion. To proxy for the tightness of the connections, we exploit the fact that two banking groups may be connected through multiple shared board members of individual banks within the group. For example, banking group A and B may be connected as bank 1 belonging to group A and bank 2 belonging to group B share a board member, but also because bank 3 belonging to group A and bank 4 belonging to group B share a board member. It is reasonable to assume that two groups have more opportunities to share information and collude if more of their constituent banks share

[33]The dummy is set to zero for control relationships, for which the network is not defined. Note that, differently for the firm characteristics, that are fixed at the firm level, network characteristics vary at the bank-province level, because the same bank belongs to different networks in different provinces (if any). This implies that the $\text{LOW}_{jp}$ is not absorbed by the dummies, so that, in addition to Treated*Post*Low, we also include all lower level interactions in the regression (unreported).

board members. We find that firms borrowing from networks with a number of within banking group connected banks above the median do record a substantially higher drop in rates in the post-period, consistent with a higher ability to collude of networks involving more connections.

Another potential determinant of network performance is the degree of symmetry of market shares between network members. Theoretically, the effect could go either way. On one hand, networks with more equal market shares might better sustain the collusion (Compte, Jenny, and Rey, 2002). On the other hand, a "leader" within the network might facilitate the emergence of leader-follower type of collusion (Mouraviev and Rey, 2011; Davies and De, 2013). In practice, Column (3) shows that there is no difference in the treatment effect according to the Herfindahl-Hirschman concentration index of bank market shares in the network. In Column (4) we use a different measure of network symmetry. Specifically, we split on the basis of the difference between the market share of the largest and the second largest bank in the network. Again, there is no evidence that this network characteristic affects the outcomes of the reform.

Another implication from theory is that collusive equilibria might be easier to sustain the greater the number of markets in which banks share membership in a network. The reason is that, in case of "defection" of a network member, the "punishment" is stronger the greater the number of markets in which it can be implemented. And, as the literature on dynamic games has documented, the larger the punishment, the higher the level of collusion that the network can sustain in equilibrium (Abreu, Pearce, and Stacchetti, 1990), and therefore the higher the drop in prices once the collusive mechanism is eliminated. We compute multi-market contacts as the average number of provinces in which each couple of banks in the network is active and create the Low dummy accordingly. The result in Column (5) weakly supports the notion that sharing membership in networks in different provinces increases the network capacity to charge higher prices: the coefficient has the expected sign and is marginally statistically significant (p-value is 0.108).

**Borrowing from interlocked banks only.** Next, we analyze the heterogeneity of the effects depending on whether firms borrow only from interlocked banks or also from non-interlocked banks. Having also non-interlocked banks as lenders could increase the bargaining power of firms against interlocked banks. If this were the case, we should see a smaller decline in rates of treated relationships of firms borrowing from both interlocked and non-interlocked banks after the reform. Moreover, among firms with only treated relationships, we expect those with only one relationship to have lower bargaining power than those with multiple relationships and therefore benefit more from the reform. To test these predictions, we modify Equation (3) as follows:

$$r_{ijpt} = \beta_0 + (\beta_1 TR_{ijp} + \beta_2 TR_{ijp} \times MAT_{ip} + \beta_3 TR_{ijp} \times MMIX_{ip}) \times POST_t + D_{ijpt} + \eta_{ijpt} \quad (4)$$

where $MAT_{ip}$ is a dummy equal to one for firms with multiple relationships, all treated, and $MMIX_{ip}$ is a dummy equal to one for firms with multiple relationships, some treated and some control. The coefficient $\beta_1$ measures the effect of the reform for firms with only one relationship, treated; $\beta_2$ the relative difference for firms with multiple relationships, all treated; and $\beta_3$ for firms with both treated and control relationships.

The results, reported in Appendix Table A5, fully align with these predictions. Column 1 reports the estimates of the specification of Equation 1 with firm-bank, bank-time, province-time, and industry-time fixed effects.[34] The interest rate for treated relationships drops by 18 basis points after the reform, in line with the saturated specifications of Table 5. When we separately estimate the effects for the three categories described above, we find that $\beta_2 = -0.325$, $\beta_3 = 0.110$, and $\beta_4 = 0.210$, with all estimates highly statistically significant. This implies that firms with less bargaining power against interlocked lenders gained the most from the reform. Still, rates on all interlocked relationships fell: we reject the hypotheses that $\beta_2 + \beta_3 = 0$ and $\beta_2 + \beta_4 = 0$.

One potential concern is that firms with only treated relationships are mechanically smaller, as the probability of having mixed relationships increases with the number of relationships, which correlates with size. We have shown that smaller firms recorded a larger drop in interest rates after the reform. To account for this potential confounding effect, in Column 3 we also include firm size, measured by log of firm assets, interacted with the post dummy. The three coefficients of interest are unaffected.

[34]This is the most saturated specification we can run. In fact, we cannot include firm-time fixed effects, which would absorb the treated dummy for firms with only interlocked relationships.

### 5.3 Spillovers on control relationships

In this section, we provide some evidence of possible spillovers from treated to control relationships. First, we analyze within-province spillovers. When a substantial share of credit in a province is supplied by network banks, non-network banks might adjust prices to take advantage of a more concentrated market structure. As a measure of exposure to spillovers, we compute the share of interlocked credit in the province in the last quarter of 2011: $Z_p = \frac{\sum_j TR_{jp} loan_{jp}}{\sum_j loan_{jp}}$, where $loan_{jp}$ is the credit of bank $j$ in province $p$ in the last quarter of 2011 and $TR_{jp}$ is a dummy equal to 1 if loans extended by bank $j$ in province $p$ are treated.

Second, we look at within-bank, across-provinces spillovers, that is, on loans issued by interlocked banks in provinces in which they are not part of any network. Having a substantial share of treated credit might affect lending strategies in provinces in which the bank is not part of a network. For instance, the bank might expand its credit supply in these provinces by cutting interest rates. As a measure of exposure to spillovers, we compute the share of a bank's interlocked credit over its total credit in the last quarter of 2011: $Z_j = \frac{\sum_p TR_{jp} loan_{jp}}{\sum_p loan_{jp}}$.

To detect spillovers, we run a regression similar to (1) using only control relationships:

$$r_{ijpt} = \theta_0 + \theta_1 POST_t + \theta_2 Z_{jp} + \theta_3 Z_{jp} \times POST_t + \boldsymbol{\theta}_4' \mathbf{X}_{ijpt} + D_{ijpt} + u_{ijpt} \tag{5}$$

where $Z_{jp}$ is either $Z_j$ or $Z_p$ and $\theta_3$ measures the extent of spillovers: when positive, an increase in the share of treated loans is related to higher rates on control loans in the post-period. Note that we cannot fully saturate this regression. In fact, when using $Z_p$, we cannot include province-time or firm-time fixed effects, while when using $Z_j$ we cannot include bank-time fixed effects. These results should therefore be taken as suggestive, as we cannot fully account for time-varying unobserved heterogeneity either at the firm or at the bank level.

Results are reported in Appendix Table A6. We estimate the most saturated specification that can identify $\theta_3$ in both regressions, that is, with firm-bank, province, and industry-time fixed effects. We find no evidence of spillovers in either dimension: in none of the specifications the estimates of $\theta_3$ are significantly different from zero. We also report the results when including bank-time effect in the within-province spillover regression and firm-time effects in the within-bank spillover regression, again finding no evidence of spillovers.

## 5.4 Firm level regressions

Our exercise so far compares treated and control relationships. While the empirical design is very robust, these estimates do not directly inform us on the overall cost of credit for a firm. In fact, this will depend on the allocation of credit between relationships, which changes over time both at the intensive (a firm can reallocate credit across credit lines) and at the extensive margin (a firm can open and close relationships). To obtain an assessment of the total effect of the reform, we estimate a specification at the firm level, rather than at the firm-bank level, to check how the average interest rate changes according to the firm's exposure to interlocked banks before the reform. We construct the (weighted) average interest rate on a firm's loans as:

$$r_{it} = \sum_j \frac{loan_{ijt}}{\sum_j loan_{ijt}} r_{ijt} \tag{6}$$

where $loan_{ijt}$ is the quantity of credit drawn in the $ij$ relationship in period $t$, and the summation is on the banks which firm $i$ borrows from. Next, we compute the share of credit that each firm obtained from treated relationships in Q4-2011:

$$ShTr_i = \frac{\sum_j TR_{ij} * loan_{ijQ4-2011}}{\sum_j loan_{ijQ4-2011}}, \tag{7}$$

where $TR_{ij}$ is a dummy equal to 1 for treated relationships. The estimating equation is:

$$r_{it} = \beta_0 + \beta_1 Post_t + \beta_2 ShTr_i + \beta_3 ShTr_i \times POST_t + \boldsymbol{\beta}_4' \mathbf{X}_{it} + Dum_i + Dum_t + \eta_{it} \tag{8}$$

Given that this regression is at the firm level, we cannot use firm-quarter fixed effects, as we would exhaust all degrees of freedom, or bank fixed effects, as the unit of observation is the firm. We therefore include firm, sector-quarter and province-quarter fixed effects and run the specification without firm and bank controls[35] (Table 10, Column 1), and including them one at a time (Columns 2-3).[36] The estimates are very stable across

[35]Bank controls are averages of the characteristics of the banks lending to the firm, weighted by the share of credit of each bank.

[36]To compute bank controls at the firm level, we take the average of the bank characteristics on individual relationships, weighted by granted credit.

specifications; the most saturated one implies that a firm borrowing only from interlocked banks would record a drop of about 30 basis points relative to one with no treated relationships. This result corroborates the hypothesis that the reform benefited firms borrowing from interlocked banks.

It is interesting to note that we obtain an estimate very similar to that in the specification of Table 5, Column (2), (-0.32). In fact, that specification is more directly comparable to the firm level estimates, as it includes firm fixed effects and firm and bank characteristics. This confirms that our preferred estimate of Column (5) of Table 5 (-0.139) is a lower bound of the overall effect: if a firm borrowing from interlocked banks also pays higher rates on loans from non interlocked banks, the fully saturated estimates will underestimate the true treatment effect. With firm level regressions, instead, these higher rates will enter the determination of the average rate paid by firms with different shares of interlocked relationships.

# 6 Credit quantity and real effects

Our focus is on prices, as our main objective is to test for the presence of collusive behavior. However, it is also interesting to ascertain the effects of the reform on credit quantity and on firm performance.

## 6.1 Credit quantity

Typically, more competition should increase supply, with positive effects on credit availability. However, the credit market differs from most other markets due to the presence of asymmetric information, which can lead to credit rationing (Akerlof, 1970; Stiglitz and Weiss, 1981). Crawford, Pavanini, and Schivardi (2018) show that rationing can be more severe in more competitive markets. We therefore control if the reform also affected quantities, given the contrasting effects of the increase in competition on credit supply. We compute the total granted credit on overdraft loans for each firm-quarter and estimate equation (8) using the log of total credit as the dependent variable. We use granted credit (as opposed to used) because it is a better measure of credit supply as it is less affected by the firm decision to use available credit.

We start by running the regression at the relationship level, using the most saturated specification. The results are reported in the first two columns of Table 11. Column (1) shows a positive (0.007) but not significant effect (p-value 0.147). In Column (2) we weigh each observation with the share of credit that the relationship accounts for at the firm level, to account for the fact that some small relationships might record large percentage changes for small variations in credit: the result is similar. We conclude that, at the relationship level, the reform had no negative effect on credit supply. This goes against the hypothesis that the breakup of IDs has reduced the information flow that banks can use to process loan applications and therefore has exacerbated problems of asymmetric information. It is consistent with the hypothesis that IDs do not play out at the level of single firm-bank relationships, the dimension necessary to affect the degree of asymmetric information on individual borrowers, but rather at the market level.

The relationship level regressions show that firms did not borrow more from treated relationships *relative to controls* after the reform. However, it might be that firms that are more dependent on treated credit increase their overall borrowing due to the increased level of competition between their lenders: that is, they might borrow more from both treated and control banks. This effect is absorbed by the firm-time fixed effects in the relationship level regressions. To check for this possibility, we run a regression at the firm level, that is, aggregating all credit across different lenders and using the share of treated credit at the firm level, analogously to what we have done in Table 10 for the interest rate.[37] Column (3) shows that, when only controlling for firm, sector-quarter and province-quarter fixed effects, the relationship is not statistically distinguishable from zero. When we control for firm characteristics (Column 4), the point estimate is very similar, while the inclusion of bank controls[38] (Column 5) raises the estimate to 0.018 and the significance to the 1% level. This implies that a firm with all credit from treated relationships recorded an increase in granted credit of around 2% compared to a firm with no treated relationships.

Overall, the loan and firm regressions agree in indicating that the reform has not led to credit restrictions possibly related to exacerbated information asymmetries: if anything,

[37] Appendix Table A7 shows descriptive statistics for granted credit at the firm level.

[38] Again, as in the previous section, these are averages across all lenders of the bank, weighted by each lender's share of credit.

credit to affected firms has increased.

## 6.2 Real effects

Our firm level analysis shows that, thanks to the severance of IDs, firms' cost of credit on treated relationships decreases by between 14 and 32 basis points while granted credit is stable (or increases slightly). Now, we study whether the positive shock to the cost of credit translates into better real outcomes by estimating firm level regressions where the dependent variable is, alternatively, the investment rate, defined as current investment over the lagged capital stock, measured at book values; the growth rate of wage bill, as proxy for employment;[39] and the growth rate of sales. Descriptive statistics for these variables are reported in Appendix Table A7. The treatment indicator is again the share of treated credit at the firm level defined in Equation 7. We control for the usual firm level time-varying variables (ROA, Liquidity, Leverage, Size, a dummy for low rating firms) and bank controls, taken as average across banks a firm borrows from (Capital ratio, Interbank funding, Liquidity ratio, ROA, Size). Note that, compared to the previous regressions, that are at the quarterly level, the performance regressions are at the annual level, as this is the frequency at which balance sheets are compiled.

Results are reported in Table 12. In the specification of Column (1) we only include firm, sector-year, and province-year fixed effects. We find that firms more exposed to treated credit recorded an increase in the investment rate after the reform. Compared to a firm with no treated relationships, a firm with all treated credit increases its investment rate by slightly more than 1 percentage point (significant at 5%), equal to 4.5% of the standard deviation of the investment rate (see Appendix Table A7).[40] Given that the share of treated credit is approximately 30%, this implies that the reform increased the aggregate investment rate of firms in our sample by 0.3% per year. In Column (2) we add firm and bank controls, finding a slightly lower coefficient (0.8 percentage points, significant at the 10% level).

In Columns (3)-(4) we find also a positive effect on labor cost, whose growth rate raises

[39] Account data do not report information on the number of employees.

[40] We assess the effects in terms of standard deviation rather than mean because the average value of the three variables is very different, ranging from 14.2% for the investment rate, -0.1% for labor costs growth and -3.1% for sales growth.

by around 0.8 percentage points for a firm with all treated credit (3.9% of the standard deviation) and by 0.22% in the aggregate. Finally, the break of banks' connections brings an increase in the growth rate of sales (1.6 percentage points for a firm with all treated credit, 6.6% of the standard deviation, and 0.65% in the aggregate). This evidence suggests that the reform had important real effects.

# 7 Conclusions

We study the effects of IDs on banks' corporate loan pricing. We use a legislative change that unexpectedly forbade IDs to test their effects on interest rates. We find that the interest rate on treated relationships declined by between 32 and 14 basis points (in our most saturated specification) relative to controls after the law became effective. We also document that the effect is stronger if the combined market share of the interlocked banks is higher, connections are tighter (more banks within the same groups are involved) and for larger and ex-ante financially stronger firms. Moreover, consistent with the prediction of models of collusion, price dispersion across loans of previously interlocked banks increases after the reform. Finally, the performance of firms more exposed to interlocked banks improved after the reform.

Our results indicate that prohibiting IDs can have pro-competitive effects. These findings can inform the policy debate on the enforcement of the existing ban in the US and on its possible adoption in the EU, where IDs are not specifically regulated but rather managed by the general competition law. They lend support to the recent emphasis that the US Justice Department has put on enforcing bans of IDs. They also indicate that stricter Antitrust policies can help to contrast the generalized reduction in competitive pressures documented by a recent body of work (De Loecker and Eeckhout, 2020; Gutiérrez and Philippon, 2017).

# Bibliography


Abreu, Dilip, David Pearce, and Ennio Stacchetti. 1990. "Toward a theory of discounted repeated games with imperfect monitoring." *Econometrica* :1041–1063.

Adams, Renée B. 2017. "Boards, and the Directors Who Sit on Them." In *The Handbook of the Economics of Corporate Governance. 1st Edition, Vol. 1*, edited by Hermalin Benjamin and Michael Weisbach. Elsevier.

Akerlof, George A. 1970. "The market for "lemons": Quality uncertainty and the market mechanism." *The Quarterly Journal of Economics* 84 (3):488–500.

Albareto, Giorgio, Michele Benvenuti, Sauro Mocetti, Marcello Pagnini, Paola Rossi et al. 2011. "The organization of lending and the use of credit scoring techniques in italian banks." *Journal of Financial Transformation* 32:143–168.

Antón, Miguel, Florian Ederer, Mireia Giné, and Martin Schmalz. 2023. "Common ownership, competition, and top management incentives." *Journal of Political Economy* 131 (5):1294–1355.

Azar, José, Martin C Schmalz, and Isabel Tecu. 2018. "Anticompetitive effects of common ownership." *The Journal of Finance* 73:1513–1565.

Azar, José and Xavier Vives. 2021a. "General equilibrium oligopoly and ownership structure." *Econometrica* 89 (3):999–1048.

———. 2021b. "Revisiting the Anticompetitive Effects of Common Ownership." CEPR Discussion Paper No. DP16612.

Benetton, Matteo and Davide Fantino. 2021. "Targeted monetary policy and bank lending behavior." *Journal of Financial Economics* 142 (1):404–429.

Bernheim, B Douglas and Michael D Whinston. 1990. "Multimarket contact and collusive behavior." *The RAND Journal of Economics* :1–26.

Bottero, Margherita, Camelia Minoiu, José-Luis Peydró, Andrea Polo, Andrea F Presbitero, and Enrico Sette. 2021. "Expansionary yet different: credit supply and real effects of negative interest rate policy." *Journal of Financial Economics* .

Brandeis, Louis D. 1914. *Other people's money and how the bankers use it.* New York: Frederick A. Stokes.

Cai, Jian, Frederik Eidam, Anthony Saunders, and Sascha Steffen. 2018. "Loan syndication structures and price collusion." Mimeo, NYU Stern.

Cai, Ye and Merih Sevilir. 2012. "Board connections and M&A transactions." *Journal of Financial Economics* 103 (2):327–349.

Chang, Ching-Hung and Qingqing Wu. 2021. "Board networks and corporate innovation." *Management Science* 67 (6):3618–3654.

Chodorow-Reich, Gabriel. 2014. "Effects of unconventional monetary policy on financial institutions." *Brookings Papers on Economic Activity* Spring:155–204.

Chuluun, Tuugi, Andrew Prevost, and John Puthenpurackal. 2014. "Board ties and the cost of corporate debt." *Financial Management* 43 (3):533–568.

Cingano, Federico, Francesco Manaresi, and Enrico Sette. 2016. "Does credit crunch investment down? New evidence on the real effects of the bank-lending channel." *The Review of Financial Studies* 29 (10):2737–2773.

Colombo, Mattia. 2022. "Board Connections and Competition in Airline Markets." PhD Dissertation, University of Mannheim.

Compte, Olivier, Frederic Jenny, and Patrick Rey. 2002. "Capacity constraints, mergers and collusion." *European Economic Review* 46 (1):1–29.

Crawford, Gregory S, Nicola Pavanini, and Fabiano Schivardi. 2018. "Asymmetric Information and Imperfect Competition in Lending Markets." *American Economic Review* 108 (7):1659–1701.

Davies, Stephen and Oindrila De. 2013. "Ringleaders in larger number asymmetric cartels." *Economic Journal* 123:F524–F544.

De Loecker, Jan and Jan Eeckhout. 2020. "The rise of market power and the macroeconomic implications." *Quarterly Journal of Economics* 135 (2):561–644.

Degryse, Hans and Steven Ongena. 2005. "Distance, Lending Relationships, and Competition." *The Journal of Finance* 60 (1):231–266.

Detragiache, Enrica, Paolo Garella, and Luigi Guiso. 2000. "Multiple versus single banking relationships: Theory and evidence." *The Journal of finance* 55 (3):1133–1161.

Dooley, Peter C. 1969. "The interlocking directorate." *The American Economic Review* 59 (3):314–323.

Faia, Ester, Maximilian Mayer, and Vincenzo Pezone. 2022. "The value of firm networks: a natural experiment on board connections." Mimeno, Tilburg Univeristy.

Faleye, Olubunmi, Rani Hoitash, and Udi Hoitash. 2011. "The costs of intense board monitoring." *Journal of Financial Economics* 101 (1):160–181.

Farrell, Joseph. 1987. "Cheap Talk, Coordination, and Entry." *The RAND Journal of Economics* 18 (1):34–39.

Fich, Eliezer M. and Anil Shivdasani. 2006. "Are busy boards effective monitors?" *The Journal of Finance* 61 (2):689–724.

Geng, Heng, Harald Hau, Roni Michaely, and Binh Nguyen. 2021. "Does board overlap promote coordination between firms?" Swiss Finance Institute Research Paper No. 21-79.

Gopalan, Radhakrishnan, Renping Li, and Alminas Zaldokas. 2022. "Do board connections between product market peers impede competition?" Available at SSRN 4053853.

Graham, Bryan S. 2020. "Network data." In *Handbook of Econometrics*, vol. 7. Elsevier, 111–218.

Green, Edward J. and Robert H. Porter. 1984. "Noncooperative Collusion Under Imperfect Competition." *Econometrica* 52 (1):87–100.

Guiso, Luigi, Luigi Pistaferri, and Fabiano Schivardi. 2012. "Credit within the Firm." *Review of Economic Studies* 80 (1):211–247.

Gutiérrez, Germán and Thomas Philippon. 2017. "Declining Competition and Investment in the US." NBER Working Paper No. 23583.

Harrington Jr, Joseph E and Andrzej Skrzypacz. 2007. "Collusion under monitoring of sales." *The RAND Journal of Economics* 38 (2):314–331.

Hauser, Roie. 2018. "Busy directors and firm performance: Evidence from mergers." *Journal of Financial Economics* 128 (1):16–37.

He, Jie Jack and Jiekun Huang. 2017. "Product market competition in a world of cross-ownership: Evidence from institutional blockholdings." *The Review of Financial Studies* 30 (8):2674–2718.

Heemskerk, Eelke M, Meindert Fennema, and William K Carroll. 2016. "The global corporate elite after the financial crisis: Evidence from the transnational network of interlocking directorates." *Global Networks* 16 (1):68–88.

Imbens, Guido and Jeffrey M. Wooldridge. 2009. "Recent developments in the econometrics of program evaluation." *Journal of Economic Literature* 47 (1):5–86.

Kandori, Michihiro and Hitoshi Matsushima. 1998. "Private observation, communication and collusion." *Econometrica* :627–652.

Khwaja, Asim Ijaz and Atif Mian. 2008. "Tracing the impact of bank liquidity shocks: Evidence from an emerging market." *The American Economic Review* 98 (4):1413–1442.

Koch, Andrew, Marios Panayides, and Shawn Thomas. 2021. "Common ownership and competition in product markets." *Journal of Financial Economics* 139 (1):109–137.

Kosekova, Kamelia, Angela Maddaloni, Melina Papoutsi, and Fabiano Schivardi. 2023. "Firm-bank relationships: a cross-country comparison." ECB Working Paper No. 2826.

Larcker, David F, Eric C So, and Charles CY Wang. 2013. "Boardroom centrality and firm performance." *Journal of Accounting and Economics* 55 (2-3):225–250.

Lewellen, Katharina and Michelle Lowry. 2021. "Does common ownership really increase firm coordination?" *Journal of Financial Economics* 141 (1):322–344.

Lotti, Francesca and Francesco Manaresi. 2015. "Finance and creative destruction: evidence for Italy." Bank of Italy Occasional Paper No. 299.

Mouraviev, Igor and Patrick Rey. 2011. "Collusion and leadership." *International Journal of Industrial Organization* 29 (6):705–717.

Nili, Yaron. 2020. "Horizontal Directors." *Northwestern University Law Review* 114 (5):1179–1262.

Paravisini, Daniel, Veronica Rappoport, and Philipp Schnabl. 2020. "Specialization in bank lending: Evidence from exporting firms." Mimeo, LSE.

Petersen, Mitchell A and Raghuram G Rajan. 2002. "Does Distance Still Matter? The Information Revolution in Small Business Lending." *The Journal of Finance* 57 (6):2533–2570.

Rajan, Raghuram G. 1992. "Insiders and outsiders: The choice between informed and arm's-length debt." *The Journal of Finance* 47 (4):1367–1400.

Rodano, Giacomo, Nicolas Serrano-Velarde, and Emanuele Tarantino. 2018. "Lending standards over the credit cycle." *The Review of Financial Studies* 31 (8):2943–2982.

Sapienza, Paola. 2002. "The effects of banking mergers on loan contracts." *The Journal of finance* 57 (1):329–367.

Schivardi, Fabiano, Enrico Sette, and Guido Tabellini. 2021. "Credit misallocation during the European financial crisis." *The Economic Journal* Forthcoming.

Sharpe, Steven A. 1990. "Asymmetric information, bank lending, and implicit contracts: A stylized model of customer relationships." *The Journal of Finance* 45 (4):1069–1087.

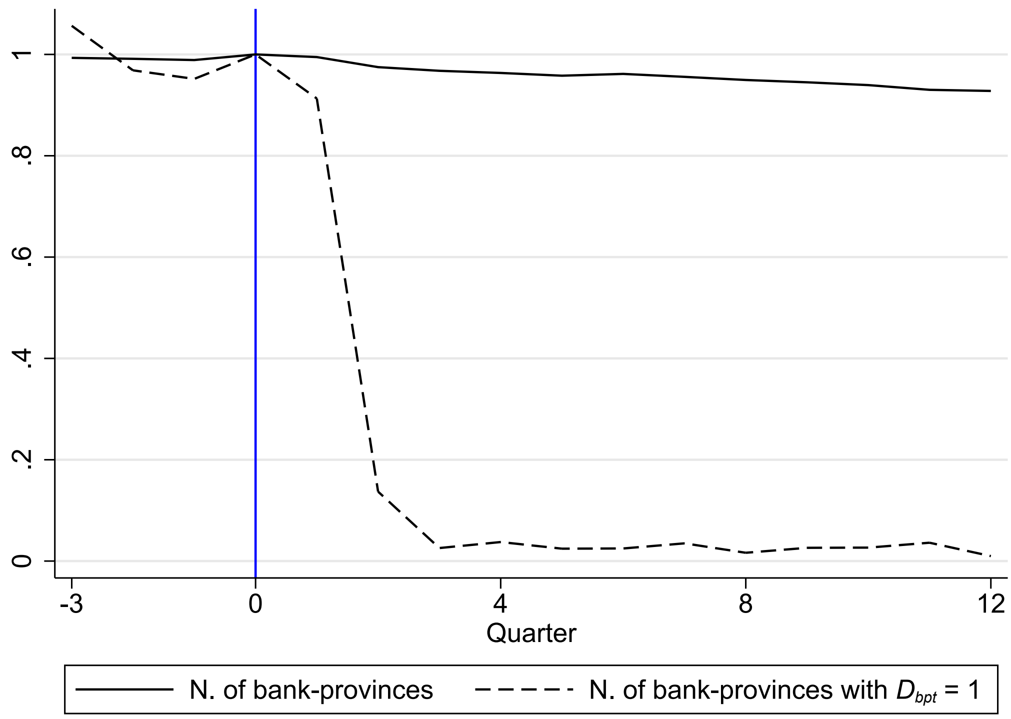
Stigler, George J. 1964. "A Theory of Oligopoly." *Journal of Political Economy* 72 (1):44–61.

Stiglitz, Joseph E and Andrew Weiss. 1981. "Credit rationing in markets with imperfect information." *The American economic review* 71 (3):393–410.

Sufi, Amir. 2009. "Bank lines of credit in corporate finance: An empirical analysis." *The Review of Financial Studies* 22 (3):1057–1088.

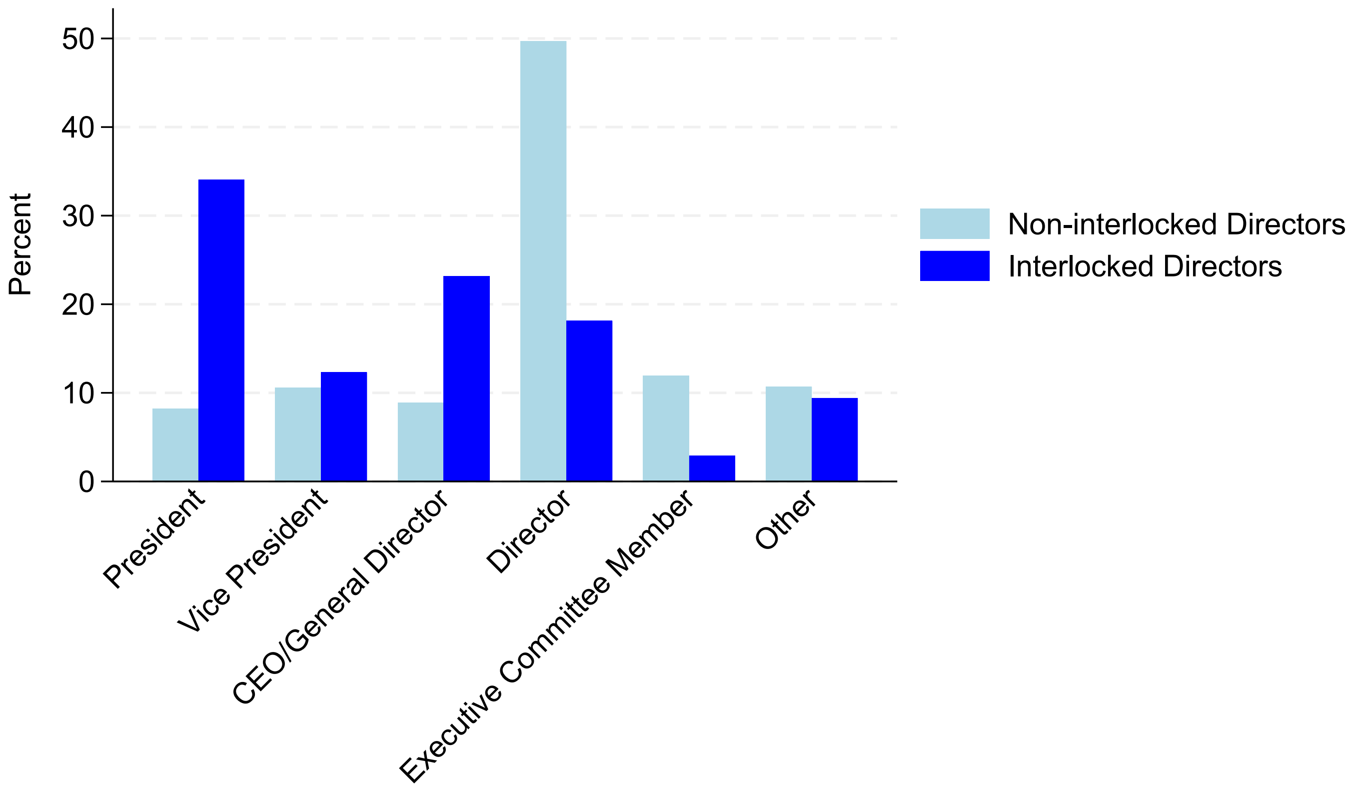
# Figure and tables

Figure 1: Bank-provinces and bank-provinces with $D_{bpt} = 1$ over time



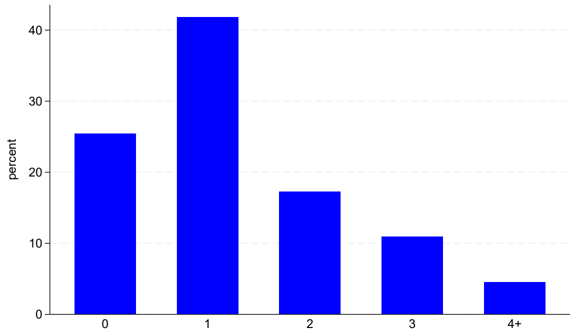

**Note:** The figure reports the evolution of the number of bank-provinces (solid line) and the number of bank-provinces with $D_{jpt} = 1$, where each bank-province is weighted by its market share (dashed line), both relative to their respective Q4-2011 value. The vertical line indicates Q4-2011, the last period before the policy. Data are at quarterly frequency from the Register of bank board members (Or.So.) and the consolidated balance sheets of the Supervisory Reports.

Figure 2: Distribution of roles

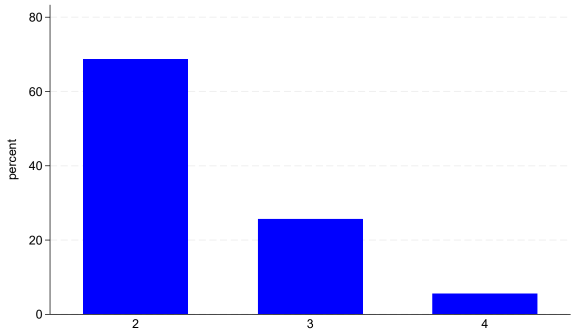



**Note:** The figure reports the distribution of roles for non-interlocked and interlocked board members as of Q4-2011.

Figure 3: Networks across provinces and their composition



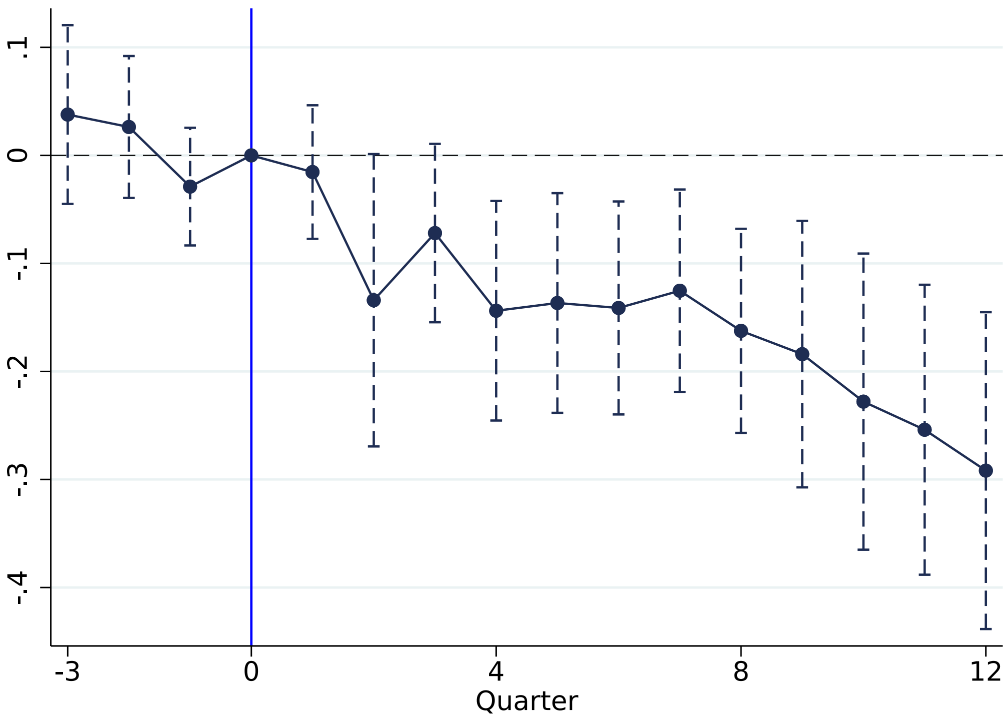

(a) Number of networks across provinces




(b) Number of banks in a network

**Note:** Panel (a) shows the distribution of the number of networks across provinces. Panel (b) shows the distribution of the number of banks across networks. Data are as of Q4-2011.

Figure 4: Evolution of the treatment effect




**Note:** The figure reports the estimated coefficients of a specification of Equation 1 in which the treatment dummy is interacted with period dummies. The dependent variable is the net interest rates on overdraft loans (revolving credit lines). The specification includes firm-quarter, bank-quarter and firm-bank fixed effects, corresponding to Column (5) in Table 5. Period zero is the baseline and corresponds to Q4-2011. Data are quarterly and the sample period goes from Q1-2011 until Q4-2014. Vertical dashed bars indicate 95% confidence intervals.

Table 1: Structure of banking systems: international comparison

| | Italy | France | Germany | Spain | UK | US |
|---|---|---|---|---|---|---|
| Total number of Credit Institutions | 754 | 660 | 1,898 | 335 | 373 | 7,357 |
| Private credit by deposit money banks to GDP (%) | 87.8 | 93.8 | 93.3 | 170.6 | 180.6 | 55.2 |
| Top 3-bank asset concentration (%) | 55.4 | 63.7 | 76.7 | 67.1 | 58.7 | 35.0 |
| Bank cost to income ratio (%) | 66.1 | 68.9 | 82.6 | 45.7 | 57.7 | 60.5 |
| Bank credit to bank deposits (%) | 123.7 | 124.0 | 127.5 | 179.6 | . | 69.9 |
| Bank net interest margin (%) | 1.7 | 0.9 | 1.0 | 1.6 | 1.8 | 3.4 |
| Bank regulatory capital to risk-weighted assets (%) | 11.4 | 12.0 | 14.8 | 11.8 | 14.4 | 13.8 |
| Average number of bank relationships | 1.95 | 1.39 | 1.59 | 1.83 | . | . |
| Share from the main bank (%) | 85.9 | 92.6 | 91.6 | 85.7 | . | . |
| Board size | 16.7 | 18.8 | 20.9 | 14.7 | 15.6 | 10.4 |
| Share of independent directors (%) | 44.8 | 34.3 | 1.9 | 46.6 | 54.4 | 77.2 |
| Share of non-executive or outside directors (%) | 78.5 | 87.3 | 76.3 | 77.5 | 69.7 | 83.8 |

**Note:** The table reports the structural characteristics of the banking systems in Italy, France, Germany, Spain, the United Kingdom (UK) and the United States (US). Total number of Credit institutions (individual banks) is taken from the European Central Bank for Italy, France, Germany and Spain, from www.statista.com for the UK, from the Federal Deposit Insurance Corporation for the US (it includes savings institutions); data refer to end-2011. Private credit by deposit money bank, Top 3-bank asset concentration, Bank cost to income ratio, Bank credit to bank deposits, Bank cost to income ratio, Bank credit to bank deposits, Bank net interest margin, Bank regulatory capital to risk-weighted assets are taken from the Global Financial Development database managed by the World Bank; data refer to the 2007-2011 average. Average number of bank relationships and Share from the main bank are based on the Eurosystem credit registry (AnaCredit) and are taken from Kosekova, Maddaloni, Papoutsi, and Schivardi (2023); data refer to 2019. Board size, Share of independent directors, Share of non-executive or outside directors are taken from Adams (2017); data refer to the 2001-2010 period.

Table 2: Board members' characteristics

| | Non-shared directors | | | Shared directors | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | N | Mean | S.D. | N |
| Female (dummy) | 0.000 | 0.000 | 138 | 0.064 | 0.244 | 6,928 |
| Graduate (dummy) | 0.540 | 0.498 | 138 | 0.384 | 0.486 | 6,928 |
| Age (years) | 64.261 | 9.652 | 138 | 60.105 | 10.656 | 6,928 |
| N. appointments | 2.072 | 0.287 | 138 | 1.000 | 0.000 | 6,928 |
| Executive role (dummy) | 0.609 | 0.490 | 138 | 0.382 | 0.486 | 6,928 |
| Duration (days) | 1,096.123 | 1,264.113 | 138 | 794.038 | 1,107.159 | 6,878 |

**Note:** The table reports descriptive statistics of board members' characteristics for those with one appointment (Non-shared directors) or more than one appointment (Shared directors) referred to Q4-2011. The dummy for executive role is equal to one for CEO, director, vice director and other top management positions. Duration is computed as the difference between December 31, 2011, and the appointment date; for those with multiple appointments, the appointment date is the first of the two appointments.

Table 3: Descriptive statistics: Networks characteristics

| | Mean | Median | S.D. | N |
|---|---|---|---|---|
| Market share (%) | 27.940 | 26.676 | 6.649 | 258 |
| Number of banks | 2.291 | 2.000 | 0.519 | 258 |
| Herfindahl index of the shares within networks | 6426 | 6213 | 1563 | 258 |
| $\Delta$ between market share of 1st and 2nd largest bank (%) | 13.696 | 13.532 | 7.284 | 258 |
| Multimarket contacts | 40.776 | 59.000 | 25.381 | 410 |

**Note:** The table shows the main characteristics of the networks. Market share is the market share of the network; Number of banks is the number of banks belonging to the network; HHI is the Herfindahl-Hirschman concentration index of markets shares of banks in the network; $\Delta_{1st-2st}$ is the difference between the market share of the largest bank in the network and the market share of the second largest bank; Multimarket contacts is the average number of provinces in which each couple of banks in the network are active. Network characteristics refer to Q4-2011.

Table 4: Descriptive statistics: Local credit market characteristics

| | Mean | Median | S.D. | N |
|---|---|---|---|---|
| N. banks | 110.227 | 97.000 | 63.552 | 110 |
| N. banks with market share $\geq 1$ | 14.336 | 14.000 | 3.556 | 110 |
| Top 3-bank market share (%) | 52.133 | 50.708 | 10.790 | 110 |
| Ln(total loans) | 22.265 | 22.119 | 1.229 | 110 |
| HHI | 1270 | 1139 | 522 | 110 |
| N. firms | 1425.5 | 963.0 | 1655.6 | 110 |
| N. banks with $D_{jpt} = 1$ | 2.939 | 3.000 | 1.081 | 82 |
| Market share banks with $D_{jpt} = 1$ (%) | 32.811 | 30.334 | 10.278 | 82 |
| N. networks | 3.146 | 3.000 | 1.198 | 82 |

**Note:** The table reports the main characteristics of local credit markets. Data refer to Q4-2011.

Table 5: Baseline regressions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Treated*Post | -0.199** | -0.317*** | -0.287*** | -0.137*** | -0.139*** |
| | (0.083) | (0.079) | (0.086) | (0.043) | (0.041) |
| Treated | 0.286*** | 0.371*** | 0.351*** | 0.218*** | |
| | (0.084) | (0.082) | (0.096) | (0.069) | |
| *Yearly control variables:* | | | | | |
| Firm | N | Y | N | N | N |
| Bank | N | Y | Y | N | N |
| *Fixed effects:* | | | | | |
| Sector-quarter | Y | Y | N | N | N |
| Province-quarter | Y | Y | N | N | N |
| Firm | Y | Y | N | N | N |
| Bank | Y | Y | Y | N | N |
| Firm-quarter | N | N | Y | Y | Y |
| Bank-quarter | N | N | N | Y | Y |
| Firm-bank | N | N | N | N | Y |
| *H0: Treated+Treated*Post=0:* | | | | | |
| F-stat | 1.549 | 0.591 | 0.591 | 1.093 | |
| p-value | 0.213 | 0.442 | 0.442 | 0.296 | |
| Observations | 3,561,068 | 3,561,068 | 2,383,776 | 2,383,748 | 2,368,426 |
| R-squared | 0.602 | 0.603 | 0.646 | 0.657 | 0.862 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2014. Treated is a dummy ($TR_{ijp}$) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. firm level control variables: ROA (EBITDA over assets), Liquidity ratio (liquidity over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Robustness to the definition of treatment

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Panel A: Market shares thresholds | | | | |
| Treated*Post | -0.127*** | -0.156*** | -0.150*** | -0.241*** | -0.130* |
| | (0.037) | (0.047) | (0.039) | (0.058) | (0.071) |
| *Thresholds:* | | | | | |
| Bank's market share | 0% | 2% | 1% | 1% | 0% |
| Network's market share | 20% | 20% | 10% | 30% | 0% |
| Observations | 2,315,014 | 2,356,111 | 2,338,379 | 2,361,858 | 2,438,647 |
| R-squared | 0.862 | 0.863 | 0.863 | 0.862 | 0.862 |
| | Panel B: Number of Pre Periods | | | | |
| Treated*Post | -0.128*** | -0.147*** | -0.133*** | -0.163*** | -0.171*** |
| | (0.039) | (0.041) | (0.038) | (0.039) | (0.042) |
| *Quarters before the treatment:* | 3 | 5 | 4 | 8 | 8 |
| *Time-varying treatment:* | N | N | Y | Y | Y |
| Observations | 2,440,232 | 2,327,139 | 2,832,938 | 3,496,156 | 2,936,260 |
| R-squared | 0.861 | 0.862 | 0.860 | 0.854 | 0.856 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2014 except for Panel B, Column 4 (from Q1-2010). Treated is a dummy ($TR_{ijp}$) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Panel A: in Column 1/2 the threshold on the single bank market share is moved to 0%/2%, respectively; in Column 3/4 the threshold on the network market share is moved to 10%/30%, respectively; in Column 5 the threshold on both market shares are moved to 0%. Panel B: in Column 1/2 we define the treatment $TR_{ijp}$ on the basis of the last 3/5 quarters before the policy; in Column 3 we use a time-varying treatment (thus all provinces are included); in Column 4 we use the time-varying treatment going back 8 periods before the enactment of the Monti Decree (thus starting in Q1-2010); in Column 5 we use the baseline treatment in 2011 (Q1 to Q4) and the time-varying treatment in 2010 (Q1 to Q4). Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Other robustness checks

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Treated*Post | -0.090*** | -0.135*** | -0.109*** | -0.120** | -0.158*** | -0.113*** | -0.130*** |
| | (0.035) | (0.040) | (0.041) | (0.057) | (0.049) | (0.041) | (0.044) |
| Observations | 2,477,122 | 2,368,426 | 1,397,271 | 592,856 | 1,958,571 | 2,053,515 | 2,628,555 |
| R-squared | 0.881 | 0.864 | 0.889 | 0.901 | 0.866 | 0.914 | 0.861 |

**Note:** The dependent variable is the net interest rate on overdraft loans (revolving credit lines) in Column 1 and the gross interest rate in all the other columns. The estimation period goes from Q1-2011 to Q4-2014 except for Columns 3-4 (see below). Treated is a dummy ($TR_{ijp}$) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Column 2 includes time-varying characteristics of the relationship as additional controls: Share bank is the share of total credit granted to the firm by the bank and Share credit line is the share of overdraft loans granted out of total loans granted within the firm-bank relationship. Column 3 restricts the sample to 4 quarters after the reform (up to Q4-2012), so as to have a symmetric pre- and post-reform period. Column 4 uses the same period of estimation as Column 3 but only uses relationships that are present throughout the entire estimation period (the closed sample). In Column 5 we drop Q1-2012 and Q2-2012. In Column 6 the regression is weighted by the contemporaneous share of drawn credit. In Column 7 two banking groups are connected if and only if the ID involves the holding banks in the two groups. Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Price dispersion

| | Before residualizing | | After residualizing | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated*Post | 0.067** | 0.100*** | 0.048*** | 0.074*** |
| | (0.028) | (0.024) | (0.027) | (0.023) |
| Treated | -0.255*** | | -0.317*** | |
| | (0.025) | | (0.024) | |
| *Fixed effects:* | | | | |
| Quarter | Y | N | Y | N |
| Province | Y | N | Y | N |
| Province-quarter | N | Y | N | Y |
| Province-treated | N | Y | N | Y |
| $H_0 : Treated + Treated * Post = 0$ | | | | |
| F-stat | 135.9 | . | 275.5 | . |
| p-value | 0.000 | . | 0.000 | . |
| Observations | 2,944 | 2,368 | 2,944 | 2,368 |
| R-squared | 0.669 | 0.848 | 0.671 | 0.851 |

**Note:** The dependent variable is the standard deviation of gross interest rate on overdraft loans (revolving credit lines) at the province-quarter-treated level. The estimation period goes from Q1-2011 to Q4-2014. Treated is a dummy equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. In Columns 3 and 4 the dependent variable is the residuals of a regression of gross interest rate on overdraft loans (revolving credit lines) on the firm level controls we use in the baseline (ROA, Liquid assets to total assets, Leverage, Log firm assets, a dummy equal 1 if the firm has a Z-score in the 3 worst categories, out of 9). Standard errors are clustered at the province-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Heterogeneity

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Panel A: Firm characteristics | | | | |
| | Size | Leverage | ROA | Liquidity | Z-score |
| Treated*Post | -0.173*** | -0.068 | -0.207*** | -0.206*** | -0.173*** |
| | (0.043) | (0.043) | (0.047) | (0.047) | (0.043) |
| Treated*Post*Low | 0.095** | -0.155*** | 0.125*** | 0.116*** | 0.127*** |
| | (0.040) | (0.036) | (0.033) | (0.036) | (0.047) |
| Observations | 2,264,014 | 2,264,014 | 2,264,014 | 2,264,014 | 2,321,182 |
| R-squared | 0.861 | 0.861 | 0.861 | 0.861 | 0.862 |
| | Panel B: Network characteristics | | | | |
| | Market share | Number of connected banks | HHI | $\Delta_{1st-2st}$ | Multimarket contacts |
| Treated*Post | -0.232*** | -0.221*** | -0.138*** | -0.163*** | -0.220*** |
| | (0.053) | (0.059) | (0.047) | (0.047) | (0.071) |
| Treated*Post*Low | 0.177*** | 0.110* | -0.003 | 0.058 | 0.115 |
| | (0.057) | (0.059) | (0.056) | (0.061) | (0.071) |
| Observations | 2,368,426 | 2,368,426 | 2,368,426 | 2,368,426 | 2,368,426 |
| R-squared | 0.862 | 0.862 | 0.862 | 0.862 | 0.862 |

**Note:** The table reports heterogeneous effects of the treatment by firm characteristics (Panel A) and network characteristics (Panel B). The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2014. Treated is a dummy ($TR_{ijp}$) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. Low is a dummy variable for values of the mediator smaller or equal to the median. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Each column considers a different mediator. Panel A: Size is the log of firm assets; Leverage is long term debt over long term debt plus equity; ROA is profits over assets; liquidity is the ratio of cash and cash equivalents to total assets (liquidity over assets); Z-score is measured on a 0-9 scale, low means that the firm has a Z-score in the third worst categories, indicating higher risk of default. Firm characteristics are measured as of end 2010. Panel B: Market share is the cumulative market share of the network; Number of connected banks is the number of banks belonging to connected banking groups that share board members; HHI is the Herfindahl-Hirschman concentration index of markets shares of banks in the network; $\Delta_{1st-2st}$ is the difference between the market share of the largest bank in the network and the market share of the second largest bank; Multimarket contacts is the average number of provinces in which each couple of banks in the network are active. Network characteristics are measured as average of the characteristics over the four quarters of 2011. Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Firm level results

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated*Post | -0.275** | -0.273** | -0.298*** |
| | (0.113) | (0.113) | (0.034) |
| *Control variables:* | | | |
| Firm | N | Y | Y |
| Bank | N | N | Y |
| Observations | 632,898 | 632,898 | 632,898 |
| R-squared | 0.800 | 0.800 | 0.800 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines), averaged at the firm level using the share of credit of each bank. The estimation period goes from Q1-2011 to Q4-2014. Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. All regressions include firm, sector-year and province-year fixed effects. firm level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the bank-province level in column 1-2 and at the firm level in the other columns. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Credit quantity

| | Loan level | | Firm level | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated*Post | 0.007 | 0.005 | 0.012 | 0.013 | 0.018*** |
| | (0.005) | (0.004) | (0.010) | (0.010) | (0.006) |
| *Control variables:* | | | | | |
| Firm | N | N | N | Y | Y |
| Bank | N | N | N | N | Y |
| Weighted regression | N | Y | N | N | N |
| Observations | 2,368,426 | 2,368,426 | 632,898 | 632,898 | 632,898 |
| R-squared | 0.969 | 0.983 | 0.949 | 0.950 | 0.950 |

**Note:** The dependent variable is the log of granted credit on overdraft loans (revolving credit lines) at the firm-bank-quarter level in Columns 1-2 and at the firm-quarter level in columns 3-5. The estimation period goes from Q1-2011 to Q4-2014. In Columns 3-5 Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. In Columns 1-2 all regressions include firm-quarter, bank-quarter, and firm-bank fixed effects; in Columns 3-5 all regressions include firm, sector-quarter and province-quarter fixed effects. In Column 2 observations are weighted by the share of used credit that the relationship accounts for at the firm level. firm level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Real effects

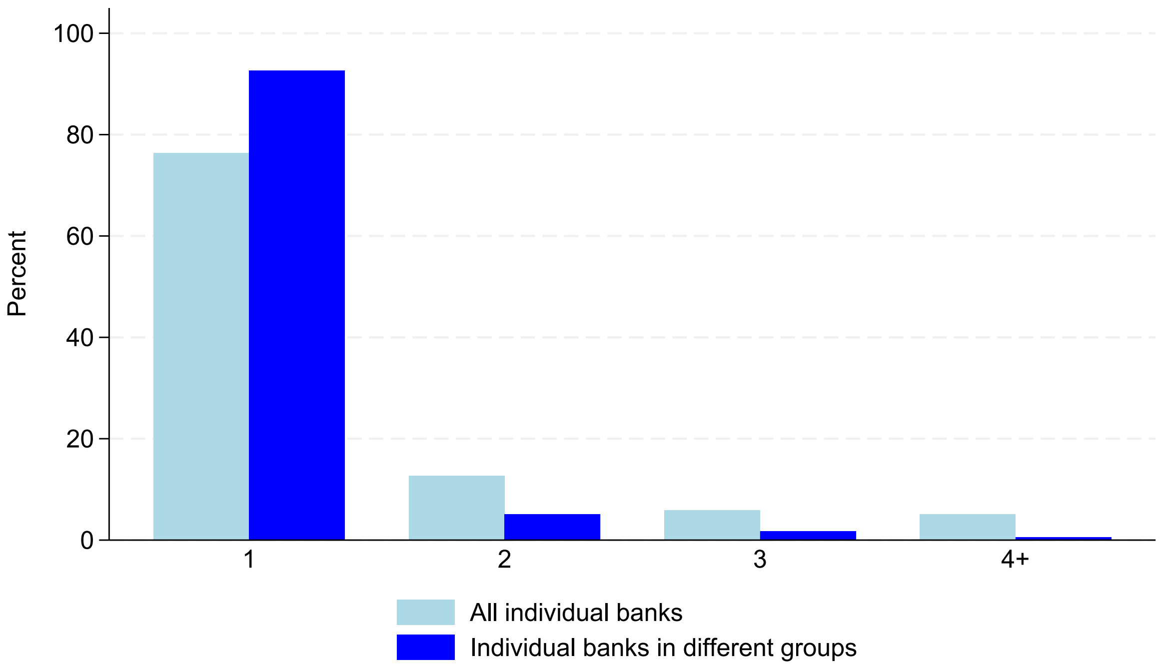
| | Investment rate | | Labor cost | | Sales | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated*Post | 1.056** | 0.843* | 0.757* | 0.779* | 1.538*** | 1.593*** |
| | (0.492) | (0.490) | (0.413) | (0.415) | (0.514) | (0.513) |
| Control variables: | | | | | | |
| Firm | N | Y | N | Y | N | Y |
| Bank | N | Y | N | Y | N | Y |
| Observations | 166,212 | 166,212 | 157,689 | 157,689 | 160,613 | 160,613 |
| R-squared | 0.467 | 0.480 | 0.422 | 0.428 | 0.406 | 0.420 |

**Note:** The dependent variable is the yearly gross investment, defined as investment over lagged total fixed assets, in Columns 1-2, the yearly growth rate of the wage bill in Columns 3-4, and the yearly growth rate of sales in Columns 5-6. The estimation period goes from 2011 to 2014 (firm balance sheet data are available at yearly frequency, therefore, for example, the investment rate of 2011 is computed as the growth rate of assets between December 2011 and December 2010. The same applies to the growth rate of the wage bill and of sales). Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify years from 2012 to 2014. All regressions include firm, industry-year and province-year fixed effects. firm level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Size (log of total assets), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# Appendixes

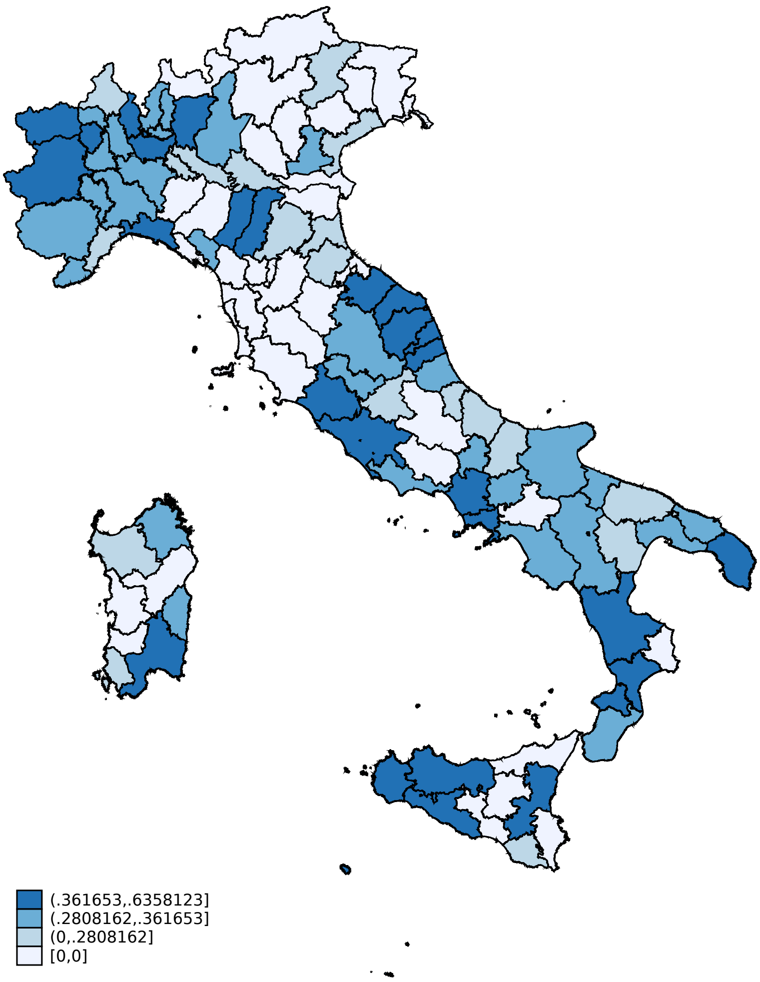
## A Additional Figures and Tables

Figure A1: Distribution of the number of SBMs



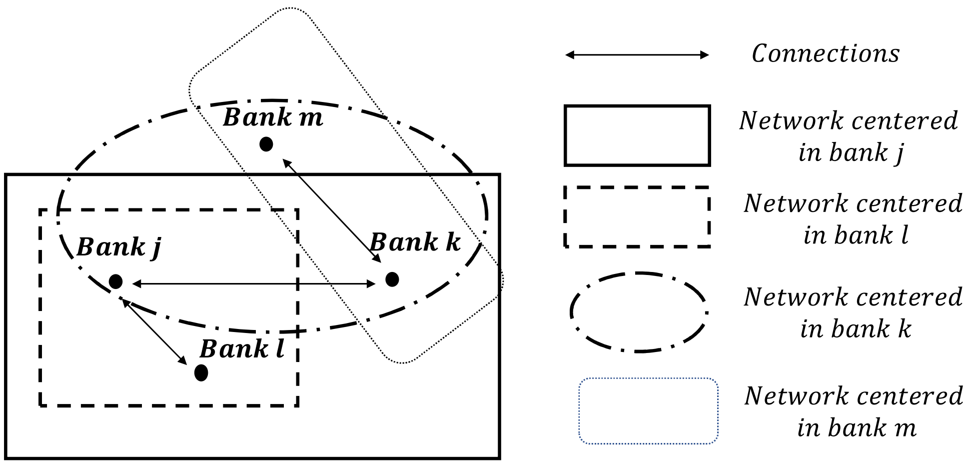

**Note:** The figure shows the distribution of the number of SBMs across pairs of connected individual banks as of Q4-2011.

Figure A2: Geographical distribution of treated relationships




**Note:** The figure shows the share of treated relationships in each province of Italy as of Q4-2011, the quarter in which the reform was passed.

Figure A3: Multiple networks




**Note:** The figure shows an example of multiple networks. Assume that the single bank market shares are above 1% and that networks' market share are above 20%. Bank $j$ is connected to banks $k$ and $l$, with which it forms a network. At the same time, bank $k$ is the center of the network including also $j$ and bank $m$; bank $l$ is connected only with bank $j$; bank $m$ is connected only with bank $k$. From the bank $j$'s viewpoint, it belongs to three networks, so that network level variables referred to bank $j$ are computed as simple averages across bank $j$'s network.

Table A1: Descriptive statistics: Bank characteristics for connected and unconnected banks

| | Connected | | | | Not connected | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Median | S.D. | N | Mean | Median | S.D. | N |
| ROA | 0.010 | 0.026 | 0.071 | 122 | 0.013 | 0.025 | 0.061 | 406 |
| Liquid Assets/Assets | 0.120 | 0.109 | 0.089 | 124 | 0.162 | 0.147 | 0.092 | 417 |
| Equity/Assets | 0.104 | 0.091 | 0.048 | 124 | 0.110 | 0.102 | 0.038 | 417 |
| Interbank Funding/Assets | 0.114 | 0.081 | 0.126 | 113 | 0.083 | 0.073 | 0.080 | 406 |
| Assets (Log) | 7.416 | 7.065 | 1.960 | 124 | 5.740 | 5.660 | 1.163 | 417 |

**Note:** The table reports descriptive statistics of banks that are connected or not as of Q4-2011.

Table A2: Interest rates - Treated and control - Pre/Post

| | Pre | | | | Post | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Median | S.D. | N | Mean | Median | S.D. | N |
| Interest rate - Treated | 9.310 | 8.987 | 3.888 | 302,338 | 10.434 | 10.001 | 4.410 | 744,513 |
| Interest rate - Control | 9.137 | 8.460 | 4.118 | 724,536 | 10.483 | 9.741 | 4.585 | 1,789,845 |

**Note:** The table reports descriptive statistics of the interest rate on revolving credit lines (overdraft facilities) for credit relationships that are treated or control. Treated relationships are defined as those with banks that have shared board members with other banks and the joint market share of these banks in the province where the borrower is located exceeds 20% in the 4 quarters prior to the reform Q1-2011 until Q4-2011). The pre-reform period spans Q1-2001 until Q4-2011. The post-reform period spans Q1-2012 until Q4-2014. The sample is restricted to banks that provide interest rate information.

Table A3: Descriptive statistics: lending relationships, firms and banks

| | Treated | | | | Control | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Median | S.D. | N | Mean | Median | S.D. | N |
| *Lending relationships* | | | | | | | | |
| Granted credit (euros) | 236,060 | 60,000 | 5,187,369 | 302,272 | 181,218 | 50,000 | 1,065,765 | 724,438 |
| Share of total credit | 0.489 | 0.391 | 0.324 | 302,272 | 0.436 | 0.321 | 0.323 | 724,438 |
| Share of credit lines | 0.315 | 0.200 | 0.299 | 302,272 | 0.308 | 0.193 | 0.300 | 724,438 |
| *Firm Characteristics* | | | | | | | | |
| ROA | 0.064 | 0.060 | 0.090 | 302,272 | 0.063 | 0.059 | 0.087 | 724,438 |
| Liquid assets/Assets | 0.046 | 0.017 | 0.073 | 302,272 | 0.043 | 0.015 | 0.070 | 724,438 |
| Leverage | 0.386 | 0.356 | 0.330 | 302,272 | 0.401 | 0.381 | 0.329 | 724,438 |
| Risky | 0.285 | 0.000 | 0.451 | 302,272 | 0.309 | 0.000 | 0.462 | 724,438 |
| Assets (Log) | 7.839 | 7.716 | 1.407 | 302,272 | 7.897 | 7.793 | 1.382 | 724,438 |
| *Bank Characteristics* | | | | | | | | |
| ROA | 0.003 | 0.002 | 0.001 | 302,272 | 0.002 | 0.002 | 0.002 | 724,438 |
| Liquid Assets/Assets | 0.078 | 0.072 | 0.022 | 302,272 | 0.078 | 0.069 | 0.044 | 724,438 |
| Equity/Assets | 0.078 | 0.079 | 0.011 | 302,272 | 0.077 | 0.077 | 0.017 | 724,438 |
| Interbank Funding/Assets | 0.066 | 0.070 | 0.034 | 302,272 | 0.096 | 0.073 | 0.086 | 724,438 |
| Assets (Log) | 19.216 | 19.355 | 1.360 | 302,272 | 17.864 | 17.889 | 2.014 | 724,438 |

**Note:** The table reports descriptive statistics of firms and banks in credit relationships that are treated or control. Treated relationships are defined as those with banks that have shared board members with other banks and the joint market share of these banks in the province where the borrower is located exceeds 20% in the 4 quarters prior to the reform Q1-2011 until Q4-2011. Descriptive statistics are shown as of Q1-2011-Q4-2011. The unit of observation is a lending relationship. Drawn credit is the amount of overdraft loans drawn (used). Share of total credit is the share of total credit granted to the firm by the bank. Share of credit line is the share of overdraft credit out of total credit supplied by the bank to the firm. ROA is EBIT over assets. Leverage is debt over equity. Risky is a dummy equal to 1 if the Altman Z-score in the three higher risk categories, out of a total of nine. The sample is restricted to banks that provide interest rate information.

Table A4: Descriptive statistics: firm characteristics according to the treatment status and number of relationships

| | Full sample | | | Two relationships | | | Three relationships | | |
|---|---|---|---|---|---|---|---|---|---|
| | Non-interl. | Mixed | Interl. | Non-interl. | Mixed | Interl. | Non-interl. | Mixed | Interl. |
| ROA | 0.07 | 0.06 | 0.07 | 0.07 | 0.07 | 0.07 | 0.06 | 0.06 | 0.06 |
| | (0.01) | (-0.04) | (0.03) | (0.00) | (0.00) | (-0.01) | (0.00) | (0.00) | (-0.01) |
| Liquid assets/Assets | 0.05 | 0.04 | 0.06 | 0.05 | 0.05 | 0.05 | 0.04 | 0.04 | 0.04 |
| | (0.04) | (-0.14) | (0.12) | (-0.01) | (0.01) | (0.01) | (-0.02) | (0.02) | (0.07) |
| Leverage | 0.39 | 0.39 | 0.36 | 0.40 | 0.39 | 0.36 | 0.40 | 0.39 | 0.36 |
| | (0.02) | (0.03) | (-0.08) | (0.04) | (-0.01) | (-0.09) | (0.03) | (-0.03) | (0.11) |
| Assets (Log) | 7.26 | 8.20 | 7.01 | 7.51 | 7.74 | 7.64 | 8.01 | 8.17 | 8.46 |
| | (-0.35) | (0.77) | (-0.44) | (-0.18) | (0.18) | (0.01) | (-0.14) | (0.12) | (0.25) |
| Risky | 0.33 | 0.27 | 0.30 | 0.32 | 0.29 | 0.26 | 0.32 | 0.28 | 0.27 |
| | (0.11) | (-0.12) | (-0.01) | (0.08) | (-0.04) | (-0.11) | (0.09) | (-0.08) | (-0.06) |
| N. of firms | 83,380 | 44,719 | 28,705 | 17,033 | 15,782 | 2,964 | 7,012 | 10,425 | 215 |

**Note:** The table reports the average and in parenthesis the normalized difference (computed as the difference between the mean of the group and the mean of the other two groups, Imbens and Wooldridge (2009)) of each firm characteristic according to the treatment status and the number of credit relationships. Non-interlocked firms are those with only control relationships; mixed firms are those with both treated and control relationships; Interlocked firms are those with only treated relationships. Treated relationships are defined as those with banks that have shared board members with other banks and the joint market share of these banks in the province where the borrower is located exceeds 20% in the 4 quarters prior to the reform Q1-2011 until Q4-2011. Descriptive statistics are shown as of Q1-2011-Q4-2011. The unit of observation is a firm. ROA is EBIT over assets. Leverage is debt over equity. Risky is a dummy equal to 1 if the Altman Z-score in the three higher risk categories, out of a total of nine. The sample is restricted to banks that provide interest rate information.

Table A5: Heterogeneous Treatment Effects: All Interlocked vs. Mixed Relationships

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated*Post | -0.182*** | -0.325*** | -0.345*** |
| | (0.0347) | (0.0404) | (0.0406) |
| Multi Rel All Treated*Post | | 0.110** | 0.124*** |
| | | (0.0436) | (0.0449) |
| Multi Rel Some Treated Some Control * Post | | 0.210*** | 0.237*** |
| | | (0.0265) | (0.0265) |
| Firm Controls | Y | Y | Y |
| Firm size*Post | N | N | Y |
| *Fixed effects* | | | |
| Firm-Bank | Y | Y | Y |
| Bank-Quarter | Y | Y | Y |
| Sector-Quarter | Y | Y | Y |
| Province-Quarter | Y | Y | Y |
| *H0: Treated*Post+Multi all Treat*Post=0:* | | | |
| F-stat | | 16.99 | 17.63 |
| p-value | | 0.000 | 0.000 |
| *H0: Treated*Post+Multi Some T Some C*Post=0:* | | | |
| F-stat | | 10.63 | 9.49 |
| p-value | | 0.001 | 0.002 |
| Observations | 3,551,658 | 3,551,658 | 3,551,658 |
| R-squared | 0.766 | 0.766 | 0.766 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2014. Treated is a dummy ($TR_{ijp}$) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. Multi Rel All Treated is a dummy equal to 1 if the firm has more than one lending relationship and all of them are treated. Multi Rel Some Treated Some Control is a dummy equal to 1 if the firm has multiple relationships and at least one of them is treated and one of them control. firm level control variables: ROA (EBITDA over assets), Liquidity ratio (liquidity over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Spillover Effects

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Share Treated Bank*Post | 0.159 | | 0.155 | 0.0280 | |
| | (0.123) | | (0.125) | (0.126) | |
| Share Treated Prov*Post | | -0.0578 | -0.0172 | | -0.0171 |
| | | (0.191) | (0.195) | | (0.0811) |
| *Yearly control variables:* | | | | | |
| Firm | Y | Y | Y | N | Y |
| Bank | Y | Y | Y | Y | N |
| *Fixed effects:* | | | | | |
| Firm-Bank | Y | Y | Y | Y | N |
| Sector-quarter | Y | Y | Y | N | N |
| Province | Y | Y | Y | N | N |
| Bank-quarter | N | N | N | N | Y |
| Firm-quarter | N | N | N | Y | N |
| Observations | 2,506,931 | 2,506,931 | 2,506,931 | 1,489,967 | 2,506,925 |
| R-squared | 0.754 | 0.754 | 0.754 | 0.859 | 0.765 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2014. Share Treated Bank is the share of treated credit in bank's portfolio as of December 2011. Share Treated Province is the share of treated credit in the province as of December 2011. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2014. firm level control variables: ROA (EBITDA over assets), Liquidity ratio (liquidity over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the bank-province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Dependent variables used in the firm level analysis

| | . Mean | Median | S.D. | N |
|---|---|---|---|---|
| Granted Credit (Log) | 12.257 | 12.206 | 1.225 | 632,898 |
| Investment Rate (%) | 14.184 | 5.155 | 23.609 | 166,212 |
| Percentage change in Labor Cost (%) | -0.097 | 1.101 | 19.380 | 157,689 |
| Percentage change in Sales (%) | -3.077 | -1.138 | 24.114 | 160,613 |

**Note:** The table reports descriptive statistics of the dependent variables used in the firm level analysis (from 2011 to 2014): the interest rate on revolving credit lines (overdraft facilities) aggregated at the firm level, using the share of credit in each credit relationship as weight; the log granted credit in the revolving credit line, the investment rate, the growth rate of the wage bill, and the growth rate of sales.

# B Additional evidence on the geographical component of the corporate lending market

We show that banks tend to specialize at the provincial level by calculating the distribution of the number of provinces in which a bank is active. We define a bank as active in a province if it has at least one branch in that province. For this analysis, we use the total population of banks operating in Italy, thus including those that do not report information on interest rates and which are therefore excluded from the estimation sample. The distribution is very skewed, with a large number of banks operating in a small number of provinces. As of December 2011, 88.8% of banks are active in at least one and at most 5 provinces, 6.3% are active between 6 and 20 provinces, 2.2% between 21 and 50, 2.0% between 51 and 100, and 0.7% in more than 100 provinces. On average, banks are active in 5 provinces, while the median is 1 and the 90th percentile is 6. Only 2 banks are active in all provinces. This supports the idea that space matters from the supply side: many banks specialize in lending in a limited number of provinces.

To analyze the bank-province component of interest rates, we run regressions of interest rates on various combinations of fixed effects, as well as on the firm-level controls we use in our baseline regression (size, leverage, ROA, liquidity, creditworthiness). The firm-level controls ensure that we compare similar firms in terms of creditworthiness. These regressions are run on the Q4-2011 cross-sectional data. The table below shows the R-squared for different combinations of fixed effects. First, there is a clear province dimension to pricing. Second, compared to including bank and province fixed effects separately, using bank*province fixed effects results in an R-squared increase of 0.023 (+18%). The gain is about half the explanatory power of all the firm-level controls. This suggests that there is a non-negligible bank-province component at work.

Table B1: R-Squared of the interest rate regressions for different sets of fixed effects

| Specification | R-Squared |
|---|---|
| No FE (only firm controls) | 0.0499 |
| Only Province FE | 0.0890 |
| Only Bank FE | 0.1291 |
| Province and Bank FE | 0.1524 |
| Province*Bank FE | 0.1759 |

**Note:** The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period is Q4-2011. All regressions include the firm-level controls used in the baseline regression (size, leverage, ROA, liquidity, creditworthiness).

Since banks might specialize in lending to firms within certain industries (because they can acquire the relevant industry-specific expertise to screen and monitor the borrowers), one could argue that an alternative definition of the relevant market could be based on industries instead of provinces. Below we provide evidence that supports the definition of markets based on the geographical rather than the sectoral dimension. To do so, we compute bank-level provincial and industrial specialization indices as follows:

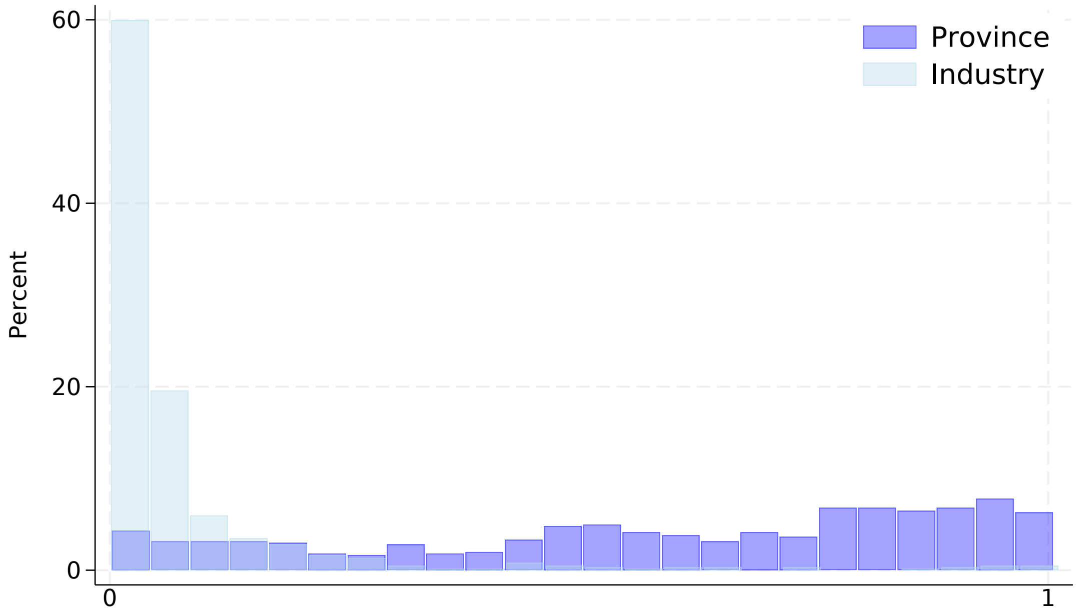
(i) Provincial specialization: $X_b = \sum_{p=1}^{110} (S_{bp} - S_p)^2$, where $S_{bp}$ is the share of province $p$ in the business loan portfolio of bank $b$ and $S_p$ is the share of province $p$ in the business loan portfolio of all banks; the sum is over the 110 provinces we have in the database. If bank $b$ has the same provincial distribution as the total one, then $X_b = 0$; if, on the contrary, bank $b$ concentrates all its loans in only one province, $X_b$ will be close to 1.

(ii) Industrial specialization: $Y_b = \sum_{k=1}^{85} (S_{bk} - S_k)^2$, where $S_{bk}$ is the share of industry $k$ in the business loan portfolio of bank $b$ and $S_k$ is the share of industry $k$ in the business loan portfolio of all banks; the sum is over the 85 industries we have in the database. If bank $b$ has the same industrial distribution as the total one, then $Y_b = 0$; if, on the contrary, bank $b$ concentrates all its loans in only one industry, $Y_b$ will be close to 1.

Note that the distributions of $X_b$ and $Y_b$ are comparable thanks to the fact that the number of provinces (110) is not very different from the number of industries (85). The two distributions are shown in Figure B1.

The distribution of provincial specialization is rather uniform, with a significant mass far from 0, indicating large deviations from the average share at the country level. This reflects the spatial specialization that we have already documented in the paper. For industries, the picture is very different, with most of the mass close to zero, suggesting that the industry composition of banks' loan portfolios is relatively close to the country average. This evidence indicates that specialization is much stronger along the spatial dimension than along the industrial dimension, reinforcing the validity of our empirical choice of the relevant market.

Figure B1: Distribution of provincial and industrial specialization across banks




**Note:** The figure shows the distributions of provincial ($X_b$) and sectoral specialization ($Y_b$) as of Q4-2011, the quarter in which the reform was passed.